Epidemic Communities

Climate change, emerging disease and the governance of science

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Abstract

Scientific knowledge is often relied upon for informing crucial societal decisions. Where this knowledge is uncertain, and/or where these decisions are made amidst a contested political landscape, science tends to become the focus of intense scrutiny, as has been evident throughout the history of climate change politics. One consequence is that instead of "scientising" decision-making, science itself becomes more explicitly politicised.

This thesis argues that in order to contribute to contemporary debates about the governance of science, it is essential to move beyond the question of whether or not policy-relevant scientific knowledge *is* credible and to examine how scientific knowledge is *made* to be credible. Drawing upon the concept of co-production and other insights from Science & Technology Studies (STS), this thesis presents a detailed examination of how research into the health impacts of climate change (infectious diseases especially) gradually gained in prominence in both public health and climate change circles. Particular analytical attention is paid to an epistemic community of climate change and health (CCH) researchers, following the ways in which they interacted with global political entities such as the World Health Organization (WHO) and the Intergovernmental Panel on Climate Change (IPCC).

Based upon in-depth interviews with actors intimately involved in CCH research, this thesis documents how the rise of CCH research influenced and was influenced by particular scientific and political contexts related to the governance of climate change as well as emerging infectious disease. The examination of a longstanding controversy surrounding CCH research reveals many socio-economic and political assumptions embedded in it, further demonstrating its contingency. However, despite that CCH research is both uncertain and contested, actors in the political world often need to know what the state-of-the-art of the field is. To examine the implications of this, the CCH controversy as treated by the assessment reports of the Intergovernmental Panel on Climate Change (IPCC) is explored. Although IPCC follows a complicated set of procedures aimed at ensuring scientific and political legitimacy, this thesis demonstrates that values and normative judgements are important components of scientific assessments, helping to co-construct particular science-policy orderings at the expense of alternative ones.

Amidst ongoing debates about how shore-up the credibility of climate change science and politics, this thesis argues that the way in which IPCC assessments are currently performed, as well as their tendency to present findings as "consensus", may undermine their political and scientific credibility.

Declaration of Originality

I hereby declare that this thesis is my own work and has not been submitted in any form for another degree or diploma at any other university. Information derived from the work of others has been acknowledged in the text and references are given in the list of sources.

Jonathan E. Suk Edinburgh, July 15, 2013

List of Abbreviations

AGGG	Advisory Group on Greenhouse Gases (UN)
ANT	Actor-Network Theory
AR4	Fourth Assessment Review (IPCC)
AR5	Fifth Assessment Review (IPCC)
BSE	Bovine spongiform encephalopathy
CCH	Climate change and health
CDC	Centers for Disease Control (USA)
CLA	Contributing Lead Author (IPCC)
CLP	Cambrosio, Limoges and Pronovost
COP	Conference of Parties (UNFCCC)
CRU	Climate Research Unit (University of East Anglia)
DALY	Disability-adjusted life year
DG SANCO	Directorate General for Health and Consumers (European Commission)
ECDC	European Centre for Disease Prevention and Control
EEA	European Environment Agency
EID	Emerging (and re-emerging) infectious disease
EP	Epidemic Potential
EPA	Environment Protection Agency (USA)
EU	European Union
FAR	First Assessment Report (of IPCC)
FDA	Food and Drug Administration (USA)
GARP	Global Atmospheric Research Programme (of WMO)
GCM	General circulation model

GHG	Global health governance
GM	Genetically modified
IAC	InterAcademy Council
IACCAA	Inter-Agency Committee on the Climate Agenda (UN)
ICD	International Classification of Diseases
ICSU	International Council of Scientific Unions
IHR	International Health Regulations
IOM	Institute of Medicine (USA)
IPCC	Intergovernmental Panel on Climate Change
LSHTM	London School of Hygiene and Tropical Medicine
MMR	Measles, Mumps and Rubella
MRSA	Methicillin-resistant *Staphylococcus aureus*
NASA	National Aeronautics and Space Administration (USA)
NIH	National Institutes of Health (USA)
NRC	National Research Council (USA)
PAHO	Pan-American Health Organization (UN)
SAR	Second Assessment Report (IPCC)
SARS	Severe Acute Respiratory Syndrome
SPM	Summary for Policymakers (IPCC)
SRES	Special Report on Emissions Scenarios
SSK	Sociology of Scientific Knowledge
STS	Science and Technology Studies
TAR	Third Assessment Report (IPCC)
TBE	Tick-borne Encephalitis
TP	Transmission Potential

UN	United Nations
UNEP	United Nations Environment Programme
UNFCCC	United Nations Framework Convention on Climate Change
USDA	United States Department of Agriculture
VBD	Vector-borne disease
WG	Working Group (IPCC)
WHA	World Health Assembly (UN)
WHO	World Health Organization (UN)
WMO	World Meteorological Organization (UN)

Introduction

From October 6-8, 2008, the Spanish Ministry for Health hosted a meeting in Madrid on behalf of the Public Health and Environment Department of the World Health Organization (WHO). This meeting, the *WHO Global Consultation on Climate Change and Health,* gathered many of the world's leading scientists and organisations relevant to the topic. Tony McMichael, intellectual pioneer of climate change and health (CCH) research and returnee to Australia after several productive years at the London School of Hygiene and Tropical Medicine (LSHTM), chaired the meeting. Kris Ebi, then of the Intergovernmental Panel on Climate Change (IPCC), acted as one of the meeting's rapporteurs. Numerous other well-known scientists attended. Howard Frumkin, then director of the National Center for Environmental Health at the US Centers for Disease Control, was there, alongside Sari Kovats, Alistair Woodward, Elisabet Lindgren and the late Paul Epstein. Each had published widely-cited studies, served on IPCC writing teams, and more generally worked to raise awareness about the neglected but important potential health impacts that could – would! – arise due to climate change (WHO, 2009).

Many of the attendees had known each other for several years, way back in the early days when the topic registered barely a blip on the WHO's radar; when funding was generally unavailable; and when "proper" public health or epidemiologic journals were highly sceptical about publishing their speculative, statistically uncertain and methodologically dubious early studies that had projected future health outcomes through the use of global climate change models.

By Madrid, however, the early days were ancient history. Climate change and, by extension, climate change and health, was on everyone's radar. The IPCC had recently published its Fourth Assessment Report and subsequently won the

2007 Nobel Peace Prize alongside Al Gore.[1] Meanwhile, the World Health Assembly (WHA), the governing body of WHO, had just passed a resolution on climate change and health, thereby creating a formal mandate for WHO to further pursue CCH work and opening up space for further funding, further research, further expert solicitations. Indeed, the very purpose of this consultation in Madrid was to identify and highlight global research priorities in CCH; the consultation was the first and most direct response the WHA Resolution. Thus buoyed by such noteworthy endorsement of the field – and through their personal invitations to Madrid, by such noteworthy endorsement of their status within this field – the conference atmosphere was triumphant and jovial. Climate change and health had "arrived".

Following Madrid, health seems to have further enhanced its position within the climate change world. After United Nations Framework Convention on Climate Change (UNFCCC) Conference of Parties 2011 meeting in Durban, South Africa (COP-17), WHO boasted that the profile of health, previously "very low" at such meetings, had gained "traction" within the UN process. WHO provides an explanation:

> While negotiations about mitigation targets and how to achieve them remains politically contentious, the health community is increasingly uniting around a series of key messages:
>
> - the necessity for broad, urgent action to reduce greenhouse gas emissions and stop continued erosion of ecosystems that sustain healthy environments and human health;
> - the need to support more climate-resilient health systems;
> - and the opportunity for smarter, more sustainable development to generate immediate health "co-benefits" in a greener, low-carbon economy.[2]

[1] http://www.nobelprize.org/nobel_prizes/peace/laureates/2007/, accessed November 20, 2012.

[2] http://www.who.int/globalchange/mediacentre/events/2011/durban_conference_update/en/index.html, accessed August 23, 2012.

The potential utility of such a message emanating from the health community has not been lost on others concerned about climate change. It is not simply that the health impacts of climate change can be cited as a good reason to mitigate greenhouse gas emissions. A group of climate change communication researchers, for example, very recently concluded that communicating climate change via a public health frame "was the most likely to elicit emotional reactions consistent with support for climate change mitigation and adaptation (Myers et al., 2012)". Positive emotional reactions could lead to more public support, which in turn could lead to additional pressure on governments to address the issue.

There are, consequently, numerous reasons for actors from political, scientific, and civil society spheres to emphasise the health impacts of climate change. It would be valid, however, to question whether or not they would be wise to do so. Could making strong claims about the health impacts of climate change turn out to be counter-productive should the claims come to be seen as mistaken? The IPCC has assessed the state-of-the-art of CCH research just as it has of atmospheric science, but how much credibility does this accrue in a post-*Climategate*[3] world? This question is further complicated by the fact that much CCH research addresses the future (e.g. more deaths *will* occur due to heat waves, or many more people *will* be exposed to malaria), even though communication and fundraising strategies, political treaties, and societal and infrastructural adaptations to climate change are all dependent on decisions that must be made in the present. And for each decision, somebody will need to determine: is CCH research credible enough to necessitate and inform action?

In this thesis, this question – relevant for all climate change science (e.g. Jasanoff, 2010) – will be asked from a somewhat different angle. Rather than considering whether CCH science *is* credible, this thesis asks: *how* is CCH

[3] *Climategate* refers to the hacking and subsequent publication of emails of scientists primarily related to the prestigious Climate Research Unit of the University of East Anglia. Chapter 5 will discuss Climategate in much more detail.

research made credible (or not)? Answers to this question will be predicated on the insight, to be elaborated upon in the next two Chapters, that much can be learned from events such as the Madrid Consultation, for it is precisely through the interactions between CCH scientists and broader political contexts that the very notion of credibility is constructed.

This thesis is organised as follows. Chapter 1 will introduce some essential background by explaining how this study fits into current discussions surrounding "expertise" and "risk". It will also provide some background on the two underlying and interconnected "risks" most relevant to the branch of CCH research that this thesis will focus on: emerging infectious diseases and climate change. In Chapter 2, the theoretical and methodological perspectives, principally from science and technology studies (STS), upon which this thesis is based will be discussed. In addition, it happens to be the case that my knowledge of the Madrid Consultation is connected to my participation in this meeting as a representative of my employer, and as a result this chapter will also explore the possible implications that this may have had on the thesis.

Chapters 3, 4 and 5 consist of three interconnected case studies. Chapter 3 focuses on the CCH research community and the factors internal and external to the field that influenced the emergence of CCH research as it transitioned from a marginal field of inquiry into a fairly prestigious one. In Chapter 4, a longstanding controversy between CCH researchers and a group of disease ecologists will be introduced and analysed. Examining this controversy exposes some deeply hidden value-based assumptions embedded in CCH research, and it also reveals some of the ways in which actors on both sides of the controversy have attempted to assert the credibility of their science. Chapter 5, meanwhile, will examine how IPCC has been influenced by its role as a boundary organisation mediating between science and politics. This context, in turn, will be drawn upon in exploring the manner in which IPCC has formally assessed the highly contested science from Chapter 4. Chapter 5 is followed by a Concluding Chapter.

Chapter 1. Expertise and the construction of global risks

1.1 Introduction

Paying attention to the role of the expert community of CCH researchers in raising the profile of public health in global climate change scientific and policy circles, this thesis draws upon the literature that has addressed the interrelationships between scientific knowledge and global governance. In section 1.2, the discussion will focus on "risk" and "expertise", highlighting the important role that expertise plays in framing initial conceptualisations of risk. To elaborate upon this discussion while providing a few concrete examples, sections 1.3 and 1.4 will explore the ways in which two purportedly "global" risks, emerging infectious diseases and climate change, have been framed and subsequently considered in international fora. The selection of these two risks for discussion is, of course, no accident, for the detailed case studies of Chapters 3-5 will primarily focus upon the CCH research that has addressed the potential impacts of climate change on infectious disease transmission. Thus sections 1.3 and 1.4 also provide important context for these chapters. Finally and more generally, the discussions throughout this Chapter will raise some more substantive questions about how, precisely, the interrelationships between expert science and governance can be both theorised and researched. This will be the focus of Chapter 2.

1.2 Expertise and the globalisation of risk

In today's "age of assessment (Rayner, 2003)", governments increasingly rely upon experts for input into technical decision-making across a wide spectrum of topics: political experts for strategizing the next election; economic experts for analysing sorts of possible causes and solutions to the financial crises of recent years; climate modellers for predicting future heat waves, earthquakes, tsunamis and floods; security experts for assessing various cyber- or nuclear-warfare risks; food-safety and genetic experts for weighing the potential health impacts from genetically-modified (GM) foods; or public health experts for tackling obesity or infectious disease.

Although experts are in high demand, outsourcing a substantial volume of policy-relevant technical work is not without its perils. At the broadest level, an emphasis on technocratic decision-making processes and highly specialised knowledge throws into question the very concept of democracy, for it limits the possibilities for what Habermas (1972) called "ideal speech" or Turner (2003) somewhat more pragmatically labelled "meaningful discussion", which are simply not possible if a given subject is incomprehensible to all but a select few:

> One assumption of meaningful discussion is some degree of mutual comprehension. But in the case of expert knowledge, there is very often no such comprehension and no corresponding ability to judge what is being said and who is saying it, and consequently no possibility of genuine 'discussion' (Ibid.: 12).

This would be true even under the ideal hypothetical circumstances that the questions asked and the experts commissioned to answer them were both perfectly objective. Yet, as we will see, these circumstances never seem to hold in practice, which raises several additional issues. To begin with, there is the problem of the framing of the questions themselves: although often portrayed as "technical", they often embed social considerations and judgements. This limits the ways in which both the issue and responses to it are considered. As Jasanoff and Wynne (1998: 5) have argued:

> …issue framings do not flow deterministically from problems fixed by nature, but also that particular framings of … problems build upon specific models of agency, causality, and responsibility. These frames are in turn intellectually constraining in that they delimit the universe of further scientific inquiry, political discourse, and possible policy options.

Technical issue framings, of course, tend to lead to a reliance on experts, but socio-political circumstances influence the ways in which expertise is sourced and mobilised by decision-makers, as several decades of science and technology studies (henceforth STS) research has demonstrated. In her landmark study of technocratic decision-making in the USA, *The Fifth Branch*, Sheila Jasanoff describes the engagement of experts by regulatory bodies as they attempt to address risk-related questions, which are invariably shrouded in uncertainty and political conflict. Although a rhetorical boundary might exist between experts and decision-makers – or between science and policy – in practice this boundary is both fluid and contested. As she wrote:

> …although the message 'leave science to the scientists' was superficially appealing, it failed to address the underlying problem of defining what counts as 'science' in areas of methodological uncertainty and political conflict (Jasanoff, 1990: 59).

Under these circumstances, different experts with their different branches of science might reasonably have come to different conclusions, and thus the selection of the experts themselves necessitates great attention: the answers that they provide, although offered in technical language, tend to implicitly incorporate "socio-politically coloured judgement(s) about the acceptability of risk (Ibid.: 232)". Because such judgements are not made explicit, expert advice can be a particularly convenient ally for technocratic decision-making bodies like the US Food and Drug Administration (FDA):

> The ambiguity of the boundary between science and policy is also strategically useful … permitting the agency to harness the authority of science in support of its own policy preferences (Ibid.*:* 178).

Unfortunately for such agencies, the ambiguity of the science-policy boundary can also be disadvantageous in those instances in which their decisions are contested. Coincident with the recognition that expertise is, like other forms of knowledge, subject to and the outcome of various social and political contexts, many have argued that there has been over the past few decades a widespread decline in the credibility of expert science. Incidents such as the scare over BSE in the UK, nuclear reactor meltdowns, or the highly visible public controversies surrounding MMR vaccines, genetically-modified foods or climate change have collectively, it has been convincingly argued, contributed to publics' mistrust of science and technology (at least in Western countries) (e.g. Hagendijk and Irwin, 2006).

A useful way for contextualising this phenomenon is through the literature on *risk*, such as Ulrich Beck's *Risk Society* (1992), in which the limitations of "expert systems" are inevitably exposed by their inability to contain risks which:

> …only exist in terms of the knowledge about them. They can thus be changed, magnified, dramatized or minimized…and to that extent they are particularly open to social definition and construction (Ibid.: 23, emphasis removed).

Categorising something as a risk may be subjective, but it nonetheless has societal implications because risks necessitate action. Collier, Lakoff and Rabinow (2004) discussed Luhmann's (1993) distinction between danger and risk. Although both refer to a potential future harm or hazard, an important difference is that if that harm is treated as *danger*, then its causes are thought to be external to human control. To treat a harm as *risk*, however, is to "*technologize* the threat, and, thus, to make our present actions responsible for it (Collier *et al.*, 2004: 5, original emphasis)". Consequently, much is at stake in how risks are framed in the first place: framings define not only the problem but also who has responsibility to manage it and what the range of suitable solutions might be.

That definitions and categorisations of risk are often, as mentioned above, performed in situations of scientific uncertainty or even controversy increases the chances that a given set of experts will be perceived as having been deficient in defining and/or containing a given risk. This potential, in turn, lays the foundation for a pluralisation of expertise – a civic revolt, of sorts, against the technocratic order of things – which Beck contemplates through his discussions on *reflexive modernisation* (cf. Beck et al., 1994) and *subpolitics*, which, among other aspects, involves the mobilisation of alternative forms of expertise and a related opening up of previously closed expert systems to new forms of political intervention.[4] Although Beck is perhaps over-stated with regards to the extent of social change that subpolitical movements might have been able to enact (e.g. VanLoon, 2002: 38-43), *Risk Society* conveniently draws attention to "expertise" and its links to the production and management of risks, both of which have also been key focal points in recent STS research. This theme is, naturally, highly relevant to this thesis, given its interest in the role that a group of experts have played in the re-framing of the way in which both emerging infectious diseases and climate change can be understood as risks. Before proceeding to discuss the ways in which experts can be said to *manufacture* risk, however, it is necessary to note that Beck's treatment of "science" and "expertise" appears to be naïve from the STS perspective. His account:

> …relies on a rather homogenized and sweeping generalist account of science and technology that is more concerned with providing a critique of an image that science constructs for itself, rather than a critique of scientific practices (VanLoon, 2002: 46)

Beck has furthermore been accused of not being adequately constructivist when it comes to the production of scientific knowledge. He maintains too much of a distinction between "nature" and "culture". As Wynne argued:

> Once one introduces the idea that scientific expert knowledge itself embodies a particular culture – that is, it disseminates and imposes particular and problematic normative versions of the human and the social – then this divide is no longer tenable (Wynne, 1996a: 75).

Attention to the ways in which scientific expert knowledge both produces and is a product of a particular (epistemic and normative) culture will be an important focal point for examining the activities of the CCH community in Chapters 3, 4 and 5. The theoretical import of this idea will be further clarified in Chapter 2, which will explore how the idiom of co-production (Jasanoff, 2006a) can both account for this observation as well as provide a workable framework for

[4] Beck (1992) also discusses the privatisation or individualisation of risk, in which individuals become more accountable for the risk-related issues that affect them personally. This, however, is beyond the scope of the current discussion.

investigating the inter-relationships between the scientific and the political worlds.

For the purposes of the current discussion, it is sufficient to note that if one accepts the proposition that expert knowledge is the embodiment of a particular culture, then one could reasonably expect, in a multi-cultural world, more than one form of expert knowledge relevant to a given problem. Where Beck appears to have assumed that alternative forms of expertise are also “technical” in nature, STS has paid great attention the alternative forms of expertise, often “local” and “lay”, that have something legitimate to contribute to risk or scientific governance. This further challenges more conventional notions about where the boundaries between science and politics lie (should they have existed to begin with!) (e.g. Bloor, 2000, Epstein, 1998, Wynne, 1996a). Work of this nature has fuelled the so-called deliberative turn in STS research, through which the democratic imperative for more and/or better participatory approaches to science governance has been made alongside critiques of the successes, failures and implications of these approaches (Hagendijk and Irwin, 2006, Irwin, 2006, Mohr, 2011, Rayner, 2003).[5] The upshot is that multiple forms of expertise have a role to play in contemporary science governance, even if the question of whether or how they should be more formally categorised and incorporated into science governance processes has been a matter of quite some contention (e.g. Collins and Evans, 2002, Jasanoff, 2003, Wynne, 2003, Durant, 2011).

More questionable than whether alternative forms of expertise have legitimate contributions to make in science governance is whether they have much influence in the production and framing of risks *before* they become a matter for science governance. To begin with, as earlier argued, risks are very much the *product* of “expert systems”. This is not simply because the applications of

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[5] Some participatory approaches have at least been half-heartedly incorporated into some governmental processes in Europe, perhaps most famously the *GM Nation?* public debate in the UK (e.g. Horlick-Jones et al., 2006, Rowe et al., 2005).

science and technology often lead to the production of risks in the first place (e.g. GM crops, anthropogenic climate change, nuclear fission), but also because the initial identification of these risks is often – certainly not always – due to the "sensory organs of science (Beck, 1992: 162)". Where scientific experts identify risks through their research, they consequently have significant sway in the initial framings of risk – embodying their particular cultures in the process.

One consequence of the influence of scientific experts in the framing of risk may be that risks tend to be viewed as "global". Beck even claimed that "risk societies bring about 'communities of danger' that can ultimately only be comprised in the United Nations (Beck, 1992: 47)". It is certainly the case that addressing risk is a key and often contentious activity at the UN level, but it would be misleading to believe that risks are inherently "global" and thus necessarily or even best dealt with internationally. To begin with, very few risks, if any, are evenly distributed across the nations and populations of the world. Furthermore, certain types of risks, notably environmental ones, tend to have been framed as "global" by actors who have something to gain from this framing, which also happens to fit neatly with the "universalising discourses" of science and globalization (e.g. Yearley, 1996: 100-141). When supranational entities like the EU or UN initiate work on a particular risk, this is not necessarily evidence that the risk is indeed universal as much as that its framing as such has been successful. From an analytical perspective, this means that problematising the ways in which risks solidify into dominant, global narratives can be an essential focus of research into global science governance processes. In turn, given that science and its practitioners are so instrumental in the *manufacturing* as well as the management of risks, they are necessarily an important focal point for investigation. It is precisely for these reasons that the CCH community, many of whom were present at the Madrid Consultation described in the Introduction, will be studied closely in this thesis (and particularly in Chapter 3).

Studying the wider interactions between experts and governance structures and processes will also be a particular focal point in this thesis. This is because these interactions play an important role in the determining *how* (and *which*) expert knowledge is made credible. As Miller and Edwards have argued:

> Science's place in global policymaking is increasingly formalized, boosting its authority in policymaking processes but also subjecting it to new forms of political and legal oversight and review. International expert institutions such as the IPCC…increasingly determine which knowledge counts and which does not, helping to shape crucial policy outcomes (Miller and Edwards, 2001: 29).

Organisations like IPCC, which are heavily reliant upon expert-driven processes, are thus excellent sites for analysing how particular understandings of risk, contingent as they are on particular standpoints, gain legitimacy where other ones do not.

In light of this discussion, the principal impetus behind this thesis should be better understood. By following a community of researchers who became instrumental in framing a particular risk – climate change and health – the intention is to understand how political and scientific contexts have influenced both the "arrival" and the "framing" of CCH. In order to provide some background contextual detail relevant to CCH, and in order to further explore how expertise and global governance processes interact, the next two sections will explore two "risks" central to this thesis, emerging infectious disease (1.3) and climate change (1.4). Section 1.5 will then summarise this Chapter before moving on to Chapter 2, which will discuss theoretical and methodological considerations.

1.3 Emerging infectious diseases and the "outbreak narrative"

Emerging infectious diseases are of great interest to this thesis given that the CCH research that will be explored in detail in Chapters 3-5 concerns the links between climate change and diseases like malaria, dengue, and tick-borne encephalitis. A discussion on EIDs provides relevant background into the scientific and political dynamics contextualising CCH research as well as, additionally, following 1.2, offering a means of providing a concrete example of how communities of experts are influential in the identification and framing of "global" risks, and how interactions between experts and policy communities are necessary for "stabilising" particular framings of these risks.

Emerging infectious diseases (EIDs) have been defined as "infections that have newly appeared in the population, or have existed but are rapidly increasing in incidence or geographic range (Morse, 1995: 7)". From roughly the mid-1980s onwards, coinciding with the emergence of HIV, EIDs have become one of the most important priorities within global public health and even global security spheres. This is in stark contrast to mid-twentieth century complacency that existed in much of the West when it was assumed that infectious diseases no longer posed a threat to humankind: "We can close the book on infectious disease", as the U.S. Surgeon General notoriously declared in 1967.[6]

Currently, according to the World Health Organization (WHO), four of the world's top ten causes of death are infectious diseases (lower respiratory infections, diarrhoeal disease, HIV/AIDS and tuberculosis), which alone account for over 16% of global deaths.[7] This proportion is even higher in low- and middle-income countries, and some estimates have suggested that infectious diseases are responsible for nearly a quarter of all human morbidity.[8] WHO has furthermore estimated that 1500 children die every hour due to infectious diseases, while the World Bank estimates that infectious diseases account for 80% of the difference in life expectancy between rich and poor countries.[9]

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[6] As cited in Kickbusch & Buse (2001).
[7] Based on 2008 data. See: http://www.who.int/mediacentre/factsheets/fs310/en/index.html, accessed May 23, 2012.
[8] Statistical Annex of 2004 World Health Report (WHO), http://www.who.int/whr/2004/en/index.html, accessed May 22, 2012.
[9]http://web.worldbank.org/WBSITE/EXTERNAL/NEWS/0,,contentMDK:20040888~menuPK:34480~pagePK:34370~theSitePK:4607,00.html, accessed June 6, 2012.

The three EIDs with the highest profiles are arguably HIV, malaria and tuberculosis, but there are countless additional examples, including methicillin-resistant *staphylococcus aureus* (MRSA), the Ebola virus, rotavirus, severe acute respiratory syndrome (SARS), and strains of influenza with "pandemic potential", such as H5N1 or H1N1. Yet even these represent only the tip of the iceberg – hundreds of additional diseases have been categorised as EIDs. One study identified 177 human pathogens as either emerging or re-emerging (Woolhouse and Gowtage-Sequeria, 2005)[10] and, similarly, an influential paper published in *Nature* identified 335 emerging disease "events" (in which EIDs originated) reported worldwide between 1940 and 2004 (Jones *et al.*, 2008).[11]

Each EID has the potential to not only cause harm to human health but also to lead to widespread moral panic, as has been seen in recent years with the recent H1N1 swine flu outbreak of 2009 or SARS in 2003 (e.g. Gilman, 2010, Suk, 2004). Particularly as concerns the latter, the way in which each disease is understood is highly significant. This is because:

> The perspective from which a disease is understood, who is threatened, who is blamed, and who is called upon to change their ways can have profound implications for what is done, and who gains or loses (Leach and Dry, 2010: 5).

Given that EIDs continue to be a focal point of attention for public health policy-makers, politicians and various publics it is, recalling the discussion from 1.2, interesting to consider the roots of the predominant framing of EIDs as an important global risk. An excellent starting point is the work of Nicholas B. King (2002), who argues that contemporary concerns with EIDs have colonial roots. His research follows the activities of a community of primarily American public health practitioners and scientists who came to develop an "emerging diseases worldview". King traces the origins of this worldview (in its contemporary

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[10] Although the authors themselves note that this number is somewhat problematic, given the lack of a consistent, "scientific" definition for emerging and re-emerging infectious diseases, the trend is well demonstrated by such a large figure.

[11] Defining what does and does not count as an EID "event" is also, unsurprisingly, problematic (e.g. Woolhouse, 2008).

formulation) to a conference jointly organized by Stephen S. Morse and Joshua Lederberg, a Nobel Prize-winning microbiologist.[12] This conference, arranged in 1989 by the U.S. National Institutes of Health (NIH) and Rockefeller University, was attended by many prominent scientists and formed the foundation from which the emerging diseases (EID) worldview expanded:

> Over the course of the next decade, the anxieties expressed at this conference would be repeated widely by its attendees, eventually hardening into an orthodox set of predictions that would later be picked up by a wider group that included other scientists, prominent journalists, local and national health officials, and, eventually, national security experts (King, 2002: 767).

In the USA, the Institute of Medicine (IOM) was particularly influential in advocating this worldview, rolling out a wide range of discipline-building activities such as organising and lobbying for funding, holding conferences, and publishing reports (King, 2004). The set of anxieties and predictions encapsulated by the EID worldview is very closely related to what Priscilla Wald coined the "outbreak narrative". This narrative:

> …links the idea of disease emergence to worldwide transformations; it interweaves ecological and socioeconomic analysis with a mythic tale of microbial battle over the fate of humanity …The outbreak narrative is a powerful story of ecological danger and epidemiological belonging, and as it entangles analyses of disease emergence and changing social and political formations, it affects the experience of both (Wald, 2008: 33).

Consistent with King (2002, 2004), one critical aspect of Wald's conceptualisation is the central role that the experts have created for themselves:

> The outbreak narrative … borrows, attests to and helps to construct expertise (Ibid.: 39).

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[12] This conference was called 'Emerging Viruses: The Evolution of Viruses and Viral Disease'. It took place 1 May, 1989 (see King, 2002, note 5).

The wider acceptance of this narrative has been accompanied by a wider influence of disease experts. As King (2004: 69) suggests, by the end of the 1990's the "basic premises of the emerging diseases campaign had gained acceptance in many American governmental agencies and international health organizations". This included not only the wider acceptance of their recommendations for suitable disease control measures (strengthened disease surveillance, better laboratory funding, enhanced professional training, etc.), but also prestigious posts for individual EID worldview scientists (Ibid.), the re-organization of agencies, such as the WHO establishment of a Division of Emerging and Other Communicable Diseases Surveillance and Control,[13] and even the creation of new ones, such as the European Centre for Disease Prevention and Control (ECDC).[14]

On the framing of the EID risk

One of the reasons that the EID worldview was so readily accepted is that the framing of the EID risk resonated with the self-interests of national and supra-national governments. In dovetailing with other prominent governmental concerns, including globalisation, security, and climate change, the EID worldview was arguably more readily accepted and more easily inserted into these ongoing processes. From its onset, the EID worldview has drawn attention to the "global" nature of the risk, as well as to the role that human activities – including ecological and environmental change – has played in the creation of the risk in the first place.

One of the very first papers in the very first issue of the now prominent journal *Emerging Infectious Diseases*[15] was authored by Stephen Morse and is titled "Factors in the Emergence of Infectious Diseases" (Morse, 1995). This has been

[13] This division is now the WHO Global Alert and Response Unit. See: http://www.who.int/csr/en/, accessed May 24, 2012.
[14] ECDC was established in 2005, two years after the global outbreak of SARS. See: www.ecdc.europa.eu, accessed September 15, 2012.
[15] The very establishment of *Emerging Infectious Diseases* by the US Centers for Disease Control and Prevention (CDC) in 1995 further demonstrates the greater attention given to the topic by governmental organisations.

followed by numerous other papers re-citing similar constellations of EID "drivers", which typically include ecological changes due to economic development and land use, changes in human demographics and behaviour, intensified global trade and travel, technological practices (e.g. organ transplants, over-use of antibiotics), microbial adaptation and breakdowns in public health infrastructures (e.g. King et al., 2006, Lederberg and Shope, 1992, Weiss and McMichael, 2004).

Given the focus of this thesis, it is especially noteworthy that climate change fits within the outbreak narrative. To begin with, King (2004: 65) notes that Morse had in his early essays acknowledged that "the concept of emergence had its intellectual roots in older understandings of environment and disease ecology," a field that can also be traced to CCH research, as will be further discussed in Chapter 3. In addition, somewhat more concretely, climate change falls under the heading "ecological change" in a table in Morse's 1995 paper; it appears in the "top ten" list of EID drivers in the widely cited paper that classified 177 pathogens as emerging or re-emerging (Woolhouse and Gowtage-Sequeria, 2005)(**Table 1**); it is a focal point of Weiss and McMichael's (2004) *Nature Medicine* essay on disease emergence; and one key climatic variable, rainfall, was highlighted alongside human population density, agriculture and antibiotic drug usage in the aforementioned paper in which 335 EID events were identified (Jones et al., 2008).

Table 1. Main categories of drivers associated with emergence and re-emergence of human pathogens [16]

Rank (according to the number of EIDs associated with them)	**Driver**
1	Changes in land use or agricultural practices
2	Changes in human demographics and society
3	Poor population health (e.g. HIV, malnutrition)
4	Hospitals and medical procedures
5	Pathogen evolution (e.g. antimicrobial drug resistance, increased virulence)
6	Contamination of food sources or water supplies
7	International travel
8	Failure of public health programs
9	International trade
10	Climate change

More broadly, as concerns the global framing of the EID risk, it is mentioned above that global interdependence is frequently cited as key factor creating or exacerbating EID risk. This is not particularly surprising – the great public health cliché, after all, as Fidler (2004b: 8) notes, is that "germs do not recognize borders". This is a common theme in the EID literature (and one repeated in the literature on global health governance and global health security) and it is one that has been carefully constructed by the architects of the outbreak narrative. Again from the US perspective, this framing was helpful for grabbing the attention of policy actors. As King (2004: 69) observes, a series of US CDC reports that drew upon the aforementioned IOM reports:

> …used scale as a resource for transforming Morse's conceptual argument into a pragmatic political campaign, providing American policy makers with a rationale for funding international health. Ostensibly "global" causes produced "local" (American) consequences…

[16] Replicated from Woolhouse and Gowtage-Sequeria (2005).

In such ways, EIDs became understood as "global rather than only a national or regional threat (Brown, 2011: 321)". Although this framing tends to claim universal vulnerability to EIDs, perhaps its most universal aspect is its applicability to policy-makers in a variety of contexts. Global public health operators, alongside American ones, have embraced the outbreak narrative and its associated anxieties surrounding globalisation. To wit, Margaret Chan, former Director-General of the WHO, wrote the following in her introduction to the WHO *World Health Report 2007*:

> …profound changes have occurred in the way humanity inhabits the planet. The disease situation is anything but stable. .. New diseases are emerging at the historically unprecedented rate of one per year. Airlines now carry more than 2 billion passengers annually, vastly increasing opportunities for rapid international spread of infectious agents and their vectors…
>
> …Traditional defences at national borders cannot protect against the invasion of a disease or vector. Real time news allows panic to spread with equal ease. Shocks to health reverberate as shocks to economies and business continuity in areas well beyond the affected site. Vulnerability is universal (WHO, 2007: 2).

The success of this "global" framing of the EID threat has, in some instances, helped to mobilise attention and resources to EIDs:

> The fear of political elites of industrialized countries was necessary for tuberculosis, a disease that had always been endemic in the developing world, to become a 'global' threat (Shiffman et al., 2002: 231).

According to King, agencies such as the US Centre for Disease Control (CDC) and World Health Organization (WHO) emphasise stopping diseases at their source due to a "postcolonial anxiety" revolving

> ... around the contamination of space itself by mobile bodies and motile environments. This is not the horror of matter (or bodies) out of place, which presupposed the identification of a place for matter; instead, it is the horror of places no longer mattering, of a 'third worlding' at home (King 2002: 773).[17]

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[17] The outbreak narrative generally depicts developing areas or the "tropics" in a somewhat less than flattering light (e.g. Brown, 2011; Wald, 2008).

In such ways, the EID worldview has been anchored in and supported by broader discourses surrounding climate change and globalisation. In an increasingly globalised world, with high volumes of trade, travel and migration, EIDs started to concern policy-makers in spheres beyond public health.

The EID worldview and health securitisation

One of the most important communities to embrace the EID worldview has been the security community, who has tended to view EIDs as an issue for national security (e.g. Brown, 2011, Feldbaum et al., 2010, Horton, 2007, McInnes and Lee, 2006). This led to a considerable amount of governmental activity addressing EIDs, which has further stabilised the central tenets of the EID worldview. As Fidler commented in 2004:

> The last decade has witnessed the previously obscure and neglected area of public health shed obscurity and neglect to become the subject matter of intense national and homeland security, foreign policy and global governance debates (Fidler, 2004c: 45-6).

As concerns the increasing attention that security circles have paid to EIDs, part of a broader trend towards the "securitisation" of global health[18], there are three principal pre-occupations: HIV/AIDS, bioterrorism agents, and rapidly spreading diseases like SARS or pandemic influenza (Feldbaum and Lee, 2004). Health securitisation has had a tangible influence on the political world. Perhaps the most famous example is the 2000 UN Security Council Resolution 1308 which formally casted HIV/AIDS as an issue for national and global security.[19] Subsequently, numerous other international initiatives have reinforced the notion of EIDs as a security threat. In 2001, the World Health Assembly (WHA), the governing body of WHO, passed a resolution on global

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[18] Although recent, health securitisation can, as Freeman (2008: 28) notes, also be seen as a return to earlier phases in the politics of health, when offices of health were subordinate to interior ministries with "their purposes lying in policing as much as policy making, their instruments typically those of surveillance and quarantine".

[19] http://daccess-ods.un.org/TMP/9518286.5858078.html, accessed May 23, 2012.

health security.[20] In 2005, there was a substantial revision and strengthening of the International Health Regulations, which among other things mandates WHO Member States to strengthen disease surveillance activities (Fidler and Gostin, 2006, Suk, 2007). At the European level, in late 2011 the European Commission announced a comprehensive set of proposals to improve health security by strengthening European preparedness to cross-border health threats.[21] [22]

Alongside such policy initiatives, there have been numerous high-profile meetings on the topic, such as those organised by *Global Health Security Initiative*, which was initiated in the aftermath of 9/11 and involves the ministers of health from several developed countries[23]. Another example is the 2006 G8 Summit in St. Petersburg, Russia, where *Fighting Infectious Diseases* was one of only four topics on the formal agenda.[24]

As the involvement of the health sector in these initiatives demonstrates, it is not simply that security and foreign policy circles appropriated the outbreak narrative to their own ends. Following 9/11, many of the pioneers of the EID worldview themselves became highly engaged in bioterrorism discussions

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[20] WHA54.14. At*:* apps.who.int/gb/archive/pdf_files/WHA54/ea54r14.pdf, accessed June 5, 2012. This Resolution "urges" WHO Member States, among other things, to begin to enhance their infectious disease surveillance, to develop or update preparedness plans, to train staff and to designate an IHR focal point. It also "requests" WHO to assist Member States with such activities.

[21] As of June, 2012, this proposal is under review by the European Council and the European Parliament. See: http://ec.europa.eu/health/preparedness_response/policy/hsi/index_en.htm, accessed June 14, 2012.

[22] The language used to articulate the necessity of the new proposals mirrors those used by Margaret Chan in the WHO 2007 Health Report. As the former European Commissioner for Health and Consumer Policy, John Dalli, stated in a press release announcing the proposals: "In today's globalised society, people and goods move across borders and illnesses can spread around Europe – and the globe - within hours. This is why the European Union and its Member States must be prepared to act together in a fully co-ordinated manner to stop a disease from spreading. The proposal we adopted today gives us the means and the structures to effectively protect our citizens across Europe from a wide range of health threats". See: http://europa.eu/rapid/pressReleasesAction.do?reference=IP/11/1516&format=HTML&aged=0&language=EN&guiLanguage=en, accessed June 14, 2012.

[23] http://www.ghsi.ca/english/index.asp, accessed June 8, 2012.

[24] http://en.g8russia.ru/docs/10.html, accessed May 23, 2012.

(King, 2004), helping to further consolidate it.[25] Likewise, the world's most prominent health agency, WHO, continued to highlight the issue following the 2001 WHA Resolution. The 2007 *World Health Report*, cited earlier, had the full title *A Safer Future: global public health security in the 21st century*, and it was prepared with the explicit aim of drawing attention to "specific issues that threaten the collective health of people internationally: infectious disease epidemics, pandemics and other acute health events".[26] This, too, can be viewed as a tactical decision aimed at solidifying the outbreak narrative and gaining further resources directed towards EID control. Lee (2010: 8) noted the success of this strategy in considering whether it might be a useful approach for other issues such as social inequalities and health:

> …the strategy of linking to other agendas is one way forward. This approach has been used, for example, to renew commitments to certain infectious diseases. Framing their prevention, control and treatment in terms of national and global security, economic development and growth, for example, has led to a proliferation of new initiatives since the late 1990s. The public health effectiveness of these efforts remains subject to debate, but greater political commitment to addressing them has been marked.

Many scholars have contemplated the implications of the turn towards health security, from varying perspectives, although these debates are beyond the scope of this discussion (cf. Brown, 2011, Elbe, 2005, Feldbaum and Lee, 2004, Feldbaum et al., 2010, McInnes, 2009, McInnes and Lee, 2006, Rushton, 2011, Heymann, 2003, Lakoff and Collier, 2008). The point here is that the securitisation of health has helped to solidify the wider acceptance of the EID worldview. This has occurred not merely because the EID worldview dovetailed with broader policy spheres and interests, but also because a community of disease experts were quite calculating with regards to how to secure greater influence for their ideas.

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[25] As King (2004: 75) further argues: "The incorporation of bioterrorism into the emerging diseases campaign sharpened two aspects of the campaign's scalar narrative. It accentuated the view that "global" causes begat "local" consequences and that international transportation, trade, and information networks threatened the health and security of the nation-state".
[26] http://www.who.int/whr/2007/en/index.html, accessed May 22, 2012.

Risk- framing and path dependencies

As argued above, the renewed dedication that governments have placed upon infectious diseases is partially because of the way in which the EID worldview has linked with broader political agendas. Yet by connecting to these agendas, the governance responses aimed at addressing EIDs are necessarily influenced by them. Under the guise of global health security, for example, the key policy reactions have tended to relate to a strengthening of “biopreparedness” for disease pandemics (Garoon and Duggan, 2008) as well as potential bioterrorism attacks (Collier et al., 2004). Another key area of activity, mentioned earlier, has been global disease surveillance, the ultimate objective being no less than an efficient “global clinic (King, 2002: 776)”, monitoring EIDs everywhere and always. All of these activities have been accompanied by a heightened interest in strengthening “global health governance” more generally, a topic which be further discussed in 2.3.

Thus particular framings of risk tend to suppress alternative ways of potentially dealing with the issue, a point many have noted with regards to EIDs:

> Popularising the concept of “emerging infectious diseases” has helped to marshal a sense of urgency but if certain populations have long been afflicted by these disorders, why are the diseases considered “new” or “emerging”? Is it simply because they have come to affect more visible – read, more “valuable” – persons? (Farmer, 1999: 39)
>
> This interlinking of disease prevention with national security is crucial because it often involves excluding people who are already socially marginalised and identifying, cordoning off, and surveilling borders adjacent or otherwise connected to the “dangerous places” they occupy (Brown, 2011: 323).

If contemporary responses to the EID risk are thus related to its framing, then it is also important to remember that central to the construction of this framing has been the activities of an expert community: very tangible political/governance responses to an issue can be (at least partially) the result of expert framings of risk. This reiterates the point that the activities of such expert communities, as well as the social and political contexts influencing these activities, are important sites for STS research interested in science-governance interactions. This will also be seen in relation to climate change (1.4).

Before moving on to discuss climate change in greater detail, one minor complaint about King's (2002, 2004) research concerning the scientists involved in creating the EID worldview needs to be raised. Although his account is self-admittedly US-centric, it nonetheless understates the role that international actors may have played in helping to forge the emerging diseases worldview. Furthermore and most crucially from the perspective of this thesis, although King focuses on the role that experts played in constructing and defining and advocating the risks posed by infectious diseases, he does not offer a close-up examination of the interactions between the EID experts and the broader science governance world.[27] With this in mind, it is time to briefly examine the role of experts in constructing the dominant narrative surrounding climate change.

1.4 Climate change: a modelled narrative

As mentioned earlier, one of the main branches of CCH research focuses on the possible impacts of climate change on the spread of EIDs such as malaria and dengue. It has been conducted primarily by epidemiologists and public health experts, and it has been embraced as an important issue by health organizations like WHO. Yet as one might expect, and as will be seen throughout Chapters 3, 4 and 5, CCH necessarily draws upon (and has benefitted from) its subscription to climate change science. It is therefore necessary to very briefly discuss the way in which climate change has been constructed and framed a "global" risk. A more in-depth discussion of the history of the close interrelationships between global climate change politics and science will occur in Chapter 5.

[27] This was not necessarily the objective of King's research, it must be acknowledged.

In 1896, the Swedish scientist Svante Arrhenius first speculated about the links between "carbonic acid" and the Earth's surface temperatures, drawing upon the work of earlier scientists including Fourier and Tyndall (Arrenhius, 1896). Just under one hundred years later, amidst growing concern about the potential implications of Arrhenius' thesis, the UN Framework Convention on Climate Change (UNFCCC) came into existence with its objective being the

> stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system.[28]

To support the UNFCCC, the Intergovernmental Panel on Climate Change (IPCC) was formally established in 1988 to provide assessments of the state-of-the-art of global climate change science. Despite – and as we will see in Chapter 5 some might even argue because of – the numerous and extensive expert assessments that have led to "consensus statements" about climate change, the topic remains a contentious one. Nearly 25 years after the establishment of IPCC, there remains the potential for controversy every time a new assessment comes out. The stances of many highly entrenched groups who have a strong interest in either debunking or promoting climate change science have not dramatically changed. Given the central role that science plays in the controversies that surround climate change, it is once again useful to examine the influence that expert communities have had in identifying and framing a global risk.

Ever since its emergence in popular and political consciousness, climate change has been viewed in global terms – which is of no surprise given that the phenomenon has often been referred to as *global warming*. The emergence of a "global" environmental consciousness began in the late 1960's (e.g. Jasanoff, 2001, Yearley, 1996b: 64-66), and it is clear that climate change has fitted within this discourse from the onset. This has also been the case in more official circles. An influential example is the 1987 UN report *Our Common Future*

[28] See Article 2. At: http://unfccc.int/key_documents/the_convention/items/2853.php, accessed May 27, 2012.

(Brundtland, 1987), which drew heavily upon "One World" thinking to emphasise the universal threats posed by a whole series of environmental problems, including a brief reference to global warming.

In more technical language, the UNFCCC defines "climate system" as "the totality of the atmosphere, hydrosphere, biosphere and geosphere and their interactions".[29] As Miller and Edwards (2001) observe, this definition contrasts with more traditional understandings of "climate", which tended to view climate as the average weather patterns that could be expected for a given location over a given period of time. Although generally agreeable to many environmentalists and international environmental policy spheres alike, the universalistic framing of the risk of climate change both reflects and reiterates the privileged role that science occupies in global climate change politics:

> …the problem of an anthropogenically enhanced greenhouse effect first came to the attention of atmospheric scientists concerned with the physics and chemistry of the climate system. From their scientific perspective, what is interesting and important about GHGs are their universal physical properties and the effects of increasing atmospheric concentrations of diffuse anthropogenic GHGs on the planet's radiation budget and thus on the climate system of the planet as a whole. Accordingly, the IPCC and the other national and international scientific bodies studying climate change have tended to regard it as a universal and global-scale problem of atmospheric emissions (Demeritt, 2001: 312).

In so far as it was science that was largely responsible for identifying the problem in the first place, this should not be surprising. Yet, just as we have seen for EIDs, although scientists have played an instrumental role in identifying and framing the risk in a particular way, it is notable that from the onset climate change science has been sponsored by national and international organisations. This history will be explored in greater detail in 5.2; to provide a brief example here, as early as 1980 the World Meteorological Organization (WMO) supported climate monitoring research, such as the World Climate

[29] At: http://unfccc.int/key_documents/the_convention/items/2853.php, accessed May 27, 2012.

Programme in 1980 and later the Global Climate Observing System. These programmes can in turn be viewed as the continuation of a much longer history of interlinkages between global atmospheric science and international organisations, who fostered international research which in turn fostered global-scale understandings of atmospheric dynamics and eventually formed the basis upon which climate change came to be understood (Miller, 2001, Edwards, 2010). Today, there exists a comprehensive international meteorological and climate modelling infrastructure, and this is to a large extent a legacy of these previous international investments. Edwards (2010) has described the mutually reinforcing relationship between this infrastructure and climate science and politics as *infrastructural globalism*:

> …projects for permanent, unified, world-scale institutional-technological complexes that generate globalist information not merely by accident … but by design. Enduring, reliable global infrastructures build scientific, social and political legitimacy for the globalist information they produce (Edwards, 2010: 25).

Climate models have a key role within this infrastructure as mediators between the "scientific" and the "political" worlds (e.g. Demeritt, 2001, Mahony and Hulme, 2011, Sundberg, 2007). One way they do so is by simultaneously validating and transforming climate data, contributing to a "global" framing of the issue. Together, climatic data and models "create a picture – a data image of the world – that is complete and whole, even though the observations are not (Edwards, 2010: 283)".[30] These pictures are used to build an understanding of how climate change could unfold, but they are also highly influential in political processes.

The relationships between climate change modelling politics are, in fact, so deeply entangled that they have been the focus of some classic STS studies demonstrating the *co-production* of science and politics (Chapter 2). One point

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[30] Edwards does however note that the practice of making global data also creates the possibility for multiple images of the world, such as through the practice of creating model "ensembles" that collectively account for the probable range of error in a given forecast period.

of focus has been General Circulation Models (GCMs). STS research on GCMs has demonstrated that there are numerous sources of contingency in their production (Lahsen, 2005, Shackley et al., 1999, Shackley and Wynne, 1995, Shackley and Wynne, 1996). To give two examples here, GCMs rely upon *parameterization*, which refers to the ways in which modellers make choices about how to represent complex processes (like cloud movement or evaporation) with exogenously specified and fixed variables. Because these choices are based upon expert judgement, this practice tends to conceal the "true" levels of variance and indeterminacy inherent to climate models (Jasanoff and Wynne, 1998).

A second example is the practice of *flux correction*, which refers to modellers' practice of overriding model calculations on energy fluxes with arbitrary values that do not necessarily correspond to any previous calculated or measured values, but which do enable model predictions to be perceived as credible. Although flux correction has been a matter of controversy within the modelling community (Shackley, 2001, Shackley et al., 1999),[31] Wynne (1996b) noted how it eventually became a legitimate practice, driven by the political expediency of being able to make long-term predictions. The outcome is that climate "scientists' perceptions of the policy process … play a role in shaping their scientific practices (Shackley et al., 1999)". Thus:

> … the *intellectual* order of global climate scientific prediction, and the *political* order of global management and universal policy control, based as it is on the promise of deterministic processes, smooth changes, long-term prediction and scientific control, mutually construct and reinforce one another (Wynne, 1996b: 371-372, original emphasis).

Such influences, although mutually stabilising, can also be mutually constraining, a point that was also made in 1.3 concerning the framing of the

[31] For example Shackley (2001) and Shackley et al. (1999) note the tensions between "purists" and "pragmatists" who have different objectives for their models and also different approaches to modelling, which to some degree appear to also relate to the organisation of different funding systems.

EID risk and the subsequent securitisation of health. In the case of climate change, it is useful to consider the example of the climate sensitivity range.[32] This is a range of global temperature increases that would be expected to occur given a doubling of atmospheric carbon dioxide concentrations. It is obtained as an output of GCMs, and has been highly influential in negotiations in UNFCCC processes. Contrary to what one might expect, and despite decades of research, the climate sensitivity range has remained surprisingly stable with time: 1.5°C – 4.5°C from 1979 through 2001, and 2°C – 4.5°C in 2007 (see Edwards, 2010, Table 14.1). The explanation again relates to the way in which GCM modellers are influenced by the political contexts in which GCM models are used. The modellers:

> … have to negotiate support and credibility for their assessment reports both with their scientific peer groups and with policy 'customers'. Their problem consists of translating scientific knowledge into a form appropriate for policy actors, while keeping favour with the surrounding research communities (van der Sluijs et al., 1998: 311)[33]

Such a dynamic thus narrows the options for the values that the climate sensitivity range considers. Van der Sluijs et al. (1998) argue that it is an *anchoring device*: a "consensus knowledge construct, interfacing between science and policy (Ibid.: 312)". One of the key features of anchoring devices is that they help to manage uncertainty because they "prevent the primary scientific case from drifting, and this serves to constrain the related policy discourse (Ibid.: 316)". Although these authors are careful to mention that such anchoring is best understood as an unintentional consequence of the broader conditions in which climate science occurs, they do suggest that because a narrowing of uncertainty occurs, global policy-making is facilitated:

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[32] IPCC defines the climate sensitivity range (more formally known as the equilibrium climate sensitivity) as: "the global annual mean surface air temperature change experienced by the climate system after it has attained a new equilibrium in response to a doubling of atmospheric CO_2 concentration". See: http://www.ipcc.ch/publications_and_data/ar4/wg1/en/ch8s8-6-2.html, accessed June 3, 2012.

[33] Wynne (1996b: 373) further supports this idea, noting that GCM modellers were concerned that predictions below 1 degree Centigrade would lead to funding cuts while predictions over 6 degrees Centigrade would "unduly frighten politicians (Wynne 1996b: 373)".

> Without such anchors there might be no coming together at all of disparate parties, and thus disintegration of any incipient policy community (Ibid.: 316).

Thus climate science is influenced by (and influences) the broader global political processes and structures surrounding climate change. Expert judgements, whether "technical", "normative", or "political", are embedded in climate models as well as model outputs, with the consequence that certain outcomes (scientific as well as political) are more likely than others.

The heavy influence of a particular set of experts in a problem as vast as global climate change can also restrict the ways in which "downstream" research takes place. One of the better examples of this has been the production and usage of the IPCC emissions scenarios (e.g. IPCC, 2000).[34] Widely used in climate change "impacts" research (such as CCH), an emissions scenario is meant to present a consistent and plausible storyline for how the socio-economic world might look in the future and, relatedly, how this would affect the volume of greenhouse gas emissions and thus the likely level of climate change. Scenarios necessarily incorporate a wide range of economic, demographic and policy assumptions that one would be quite right to assume is well beyond the formal expertise of most climate modellers. Yet although official IPCC scenarios published in 1990, 1992 and 2000 were negotiated and approved intergovernmentally (Girod et al., 2009) – adding a clear political dimension to scenario production[35] – it is not necessarily the case that more diverse forms of expertise are substantially considered when scenarios are constructed. As Hulme and Dessai (2008: 67) commented regarding the production of national emissions scenarios for the UK:

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[34] The IPCC emissions scenarios were highly influential in climate change research throughout the 1990s and 2000s. As IPCC is working towards its 5th Assessment Report, it has now effectively delegated the production of scenarios to the research community—it is no longer leading the development of these scenarios.

[35] Hulme and Dessai (2008: 68) note: "Climate scenarios are the result of an increasingly intricate negotiation between scientists, policy-makers, communicators and numerous diverse stakeholders in society".

> There is a tendency, emerging from the epistemological hegemony of natural science-based climate models over other approaches to portraying the future, that debates about scenario construction revolve around technical details – spatial downscaling, construction of probabilities, temporal resolution, more climate variables – rather than around different ways of seeing world futures or of articulating the particular decision-contexts in which scenarios will be used…

Critiques along these lines have also been made about the IPCC process for developing its Special Report on Emissions Scenarios (SRES) (IPCC, 2000). Most prominently were claims that value-laden economic and demographic assumptions informed the SRES process. This led to a highly visible controversy on the topic (Castles and Henderson, 2003b, Castles and Henderson, 2003a, Schenk and Lensink, 2007), through which it was observed:

> …SRES is suggestive in representing the worldview of certain parties, and to favour certain parties' solutions for problems (Schenk and Lensink, 2007: 298).

One way this occurred was through a set of assumptions surrounding "convergence", which refers to a closure of the distance of the income gaps between rich and poor countries. As the UK House of Lords concluded in its review of the topic: "scenarios in which limited convergence took place would be politically difficult for IPCC to contemplate… (House of Lords, 2005: 37)". The review thus noted:

> We have some concerns about the objectivity of the IPCC process, with some of its emissions scenarios and summary documentation apparently influenced by political considerations (House of Lords, 2005: 6).

A consequence of having a specific set of assumptions surrounding "convergence" is that the range of deliberation was narrowed, as only some potential futures are considered (Oppenheimer et al., 2007). This impacts not only the scope of political negotiation over mitigating climate change, but also "downstream" climate change impacts research, for in these areas, like CCH, the *outputs* from GCMs and climate scenarios are used as *inputs* to their work. As this occurs, impacts researchers necessarily incorporate the assumptions of these models into their own work – thus lending further credibility to them (e.g. Demeritt, 2001). This is can be problematic:

> The IPCC has not demonstrated that the SRES emission projections have a sound economic foundation. Because these emissions projections are used as inputs in models of temperature and climate impacts, these in turn do not have a sound economic basis (Stegman, 2006: 5).

Whether or not a "sound" basis for future scenarios can ever be determined is a question that might also have been asked. The point to be made here is that models of potential climate change impacts have a *contingent* socio-economic basis, one emanating from the dominant climate change narrative and its related political order. Just as we have seen in 1.3, the relationships between particular expert communities and the broader political world lead to the creation of "dominant" narratives. This limits the possibility for alternative understandings of the problem. Indeed, many have noted that the framing of climate change as depicted here has been at

> the expense of other ways of formulating the problem, such as the structural imperatives of the capitalist economy driving those emissions, and indeed of other problems, such as poverty and disease (Demeritt, 2001: 312).

It has been famously argued, for example, that the presentation of climate change as a "global" problem tends to gloss over important considerations about the equity of historic and future greenhouse gas emissions (Agrawal and Narain, 1991). Despite such examples, it has conversely been argued that the social sciences, including science and technology studies, have not been assertive enough in presenting alternative interpretations of climate change science. One reason for this could be the fear that deconstructivist accounts of the politics and science of climate change could play into the hands of climate skeptics (Grundmann & Stehr, 2010). The danger of absenteeism is, however, likely as problematic as engagement. Where important normative or ethical issues find no room for discussion in the formal technical discourse, controversies can spill over into the science: precisely because narratives of risk are so heavily built upon technical understandings, one of the main recourses for opponents of a particular risk framing is to challenge the scientific basis of that framing.

Heavily predicated on the technical authority of "science", the dominant narratives surrounding climate change can be substantially undermined when flaws in the science are exposed, which is not particularly difficult to do for such highly complex and uncertain topic. This is a point that will be revisited in Chapters 4 & 5.

1.5 Summary

This Chapter has highlighted the role of scientific experts in the identification, framing and subsequent management of "global" risks. It was noted that knowledge is heavily relied upon in global governance systems. Experts, as the discussions on EIDs (1.3) and climate change (1.4) have demonstrated, are highly influential in developing the dominant narratives surrounding risks, and in constructing these narratives, their expertise is further privileged, potentially over alternative forms of expertise. In this way, highly technical understandings of risks emerge, narrowing the subsequent discussion about what a risk is, who is affected by it and how it could be best contained.

Meanwhile, social and political contexts influence the ways in which experts identify and frame a given problem, which in turn influences the governance responses developed to manage these problems. In the case of EIDs (1.3), experts worked extensively to promote the "outbreak narrative" which eventually became prominent in the highest echelons of domestic and global health circles. Their worldview seems to have gained further traction when it dovetailed with the interests of security circles, thereby leading to a further stabilisation of the EID worldview and its associated governance responses such as the revamped International Health Regulations and enhanced global disease surveillance. As concerns climate change (1.4), it was pointed out that the very identification of the issue was dependent on the globalist infrastructure for meteorology already in place. This served not only to foster a "global" framing of the problem but also to privilege very particular forms of scientific expertise, heavily based upon the atmospheric sciences and the climate models that they produce. At the same time, however, expert judgements are "imported" into climate change models, upon which communities such as CCH researchers are dependent. In this way, the dominant narrative surrounding climate change is further stabilised.

Given the discussion of this Chapter, it should be apparent that the interactions between expert scientists and global governance structures are an interesting and important site for studies of global risk and of global governance. This will be further explored in Chapter 2, where it will be noted that much of the literature on these topics have tended to not adequately problematise "science". STS research, meanwhile, offers a valuable suite of analytical perspectives for addressing this topic, but there appear to be comparatively few empirically-rich analyses of international communities of scientists and their interactions with global political structures.

On the organisation of this thesis

Recall that the central question of this thesis is: *how* has CCH research been made credible (or not)? As I have suggested, answering such a question necessitates studying the scientists involved in CCH research and their interactions with broader political structures and processes.

Drawing upon STS research and the idiom of co-production in particular, Chapter 2 will outline an epistemological and practical framework for pursuing the central questions of this thesis. This Chapter will also explain how my own particular normative and political commitments could have affected this research. Thereafter, Chapters 3, 4, and 5 will present three interrelated case studies related to CCH research and heavily based upon extensive interviews with leading actors in the field. In Chapter 3, the CCH community described in

1.1.1 will be more formally introduced. The underlying motivations and worldviews informing their research, and their discipline-building activities will be closely followed to demonstrate the ways in which CCH "arrived" and was framed as a global health risk. As mentioned earlier, however, the framings of risk inevitably mean that alternative ones are neglected. In Chapter 4, the implications of this will be explored in quite some detail through a longstanding controversy between the CCH community and a group of ecologists who fiercely resisted the findings and implications of CCH research. A careful examination of this controversy reveals the tacit and normative assumptions embedded within CCH research, thereby demonstrating the contingency of the findings in the field. In addition, this Chapter argues that subscribing to the pre-existing climate change narrative depicted in 1.4, while broadly convenient for the CCH community, was not without its perils.

In contrast with the controversy depicted in Chapter 4, CCH is, as per Chapter 3, relatively more "stable" in the global governance world. In order to understand how this is so, it is instructive to focus upon the ways in which CCH knowledge has been "warranted" in official channels. Thus, Chapter 5 revisits the controversies from Chapter 4 by examining how the IPCC has treated them in its previous four assessment reports. Although many of the same protagonists and antagonists from Chapters 3 and 4 reappear in Chapter 5, the IPCC portrayal of these controversies reveals the ways in which the IPCC's complicated history and procedures affect the contents of its assessments. Following Chapter 5, the concluding Chapter will then discuss the implications of some of the key insights from this thesis.

Chapter 2. Science and its governance: theoretical and methodological considerations

"When science gets into politics, the truth tends to go out the window" – Interviewee

2.1 Introduction

Having introduced the focus of this thesis and discussed the way experts influence the identification, framing and management of risks, it is necessary to further articulate the analytical perspectives through which the relationship between science and its governance can be understood. This necessitates elaborating upon three items in particular: what is implied by the literature on *governance* and its conceptualisation of the role of expertise (2.2); how contemporary accounts describe the current *global health governance* landscape (2.3); and how mutually constitutive interactions between science and global governance can be theorised and studied from an STS perspective (2.4). Following these discussions, 2.5 will discuss the epistemological and reflexive considerations related to these analytic perspectives, and 2.6 will describe the research strategy employed in this thesis.

2.2 Global governance, epistemic communities and science studies

Governance, a "rather promiscuous concept (Newman, 2001: 12)", sometimes appears to as many definitions as it does analysts. Kooiman (2003: 4) defined it as the "totality of theoretical conceptions on governing"; Rhodes (1997: 46) defined it as "the new method by which society is governed"; and Rosenau (1992: 4) defined it as "activities based on shared goals". These definitions are, of course, not necessarily mutually exclusive. Perhaps the largest consistency within the literature is that *governance* emphasises a transition away from *government*. A wider constellation of actors and influences are involved and, consequently, the act of governing has become decentralised. Governments attempt to steer policy processes and outcomes but they must do so within decentralised networks and amidst changing constellations of power (e.g. Irwin, 2008, Kooiman, 2003).

The concept of governance is particularly suitable for the "global" level; the literature on global governance tends to highlight a relocation of authority from a state-centric to a more complex political system, in part due to years of neo-liberal reform (Held et al., 1999). This transition is chaotic, disaggregated, unevenly spread, and constantly shifting. Power has dispersed upwards to supra-national organizations such as the World Bank, World Trade Organization or European Union, downwards to sub-national levels, and sideways to non-governmental actors.

It is helpful to follow Freeman's (2008) differentiation between the empirical, normative, and analytical dimensions of governance. The remainder of this section will address the normative and analytical dimensions, whereas an empirical dimension will be discussed in 2.3. To start with the normative, it is important to note that there is a need for caution for avoiding the "ideological baggage (Irwin, 2008: 585)" surrounding governance. It has been pointed out, for example, that governance has been not unproblematically linked to discourses surrounding *globalization*[1]; that it has, in some applications, certain embedded meanings with regards to what the role (or size) of government *should* be; and that it tends to depoliticise the nature of government through its rather managerial and technical discourse (Irwin, 2008; Freeman, 2008).

For these reasons, it is preferable, from an STS perspective, to be somewhat critical about the literature on governance, a point best elaborated upon by

[1] Globalization is a variable and contested term. Some have stressed that globalisation leads to a blurred distinction between global and local, and it occurs not only between but also within nation-states. It is also an uneven process - its positive and negative consequences are not evenly distributed throughout the world and, moreover, the degree to which regions of the world are interconnected and interdependent varies (e.g. Beck, 2002, Giddens, 1990).

turning to its analytical dimension. Particularly relevant for STS is the way governance theory draws attention to the role of actors and their knowledge. It tends to suggest that information has dispersed alongside power, making knowledge an essential resource:

> No single actor, public or private, has the knowledge and information required to solve complex, dynamic and diversified societal challenges; no governing actor has an overview sufficient to make the necessary instruments effective; no single actor has sufficient action potential to dominate unilaterally (Kooiman, 2003: 11).

In this situation, institutions must turn somewhere to source the information relevant to the problems that concern them. Thus cooperation amongst actors is required, but for that to happen actors must have at least some agreement concerning what the issues are and how they should be addressed. As Freeman (2008: 32) has noted:

> A precondition for the concept and practice of governance is that an issue must be of concern to a range of actors whose interests and objectives must sufficiently converge to enable cooperation.

A central question that this raises is how to understand and analyse such "convergence". Recalling the discussion from 1.2-1.4, I would suggest that studying "convergence" requires an understanding of the common narratives that actors subscribe to, which in turn requires an examination of experts, their interests, and their role in creating such narratives. Although the governance literature has paid great attention to this, its accounts tend to neglect the findings from STS. Numerous political scientists, from various perspectives, have attempted to conceptualise the formative role that scientific knowledge and expertise play in policy-making. This has principally come out of studies of environmental politics. Prominent work includes research into epistemic communities (Haas, 1992), advocacy coalitions (Sabatier and Jenkins-Smith, 1993) and discourse coalitions (Hajer, 1995).

It is of particular interest to this thesis to focus on the concept of epistemic communities, which simultaneously draws attention to the role of experts within governance structures while also serving to demonstrate how STS can contribute analytically to this literature.

Haas (1992) described epistemic communities as networks of professionals with expertise in a specific area, sharing similar normative and causal beliefs and a common policy enterprise. The influence an epidemic community has and the ideas it develops, Haas suggests, can be mutually reinforcing. Power lies in knowledge, and knowledge is stabilised by political acceptance:

> It is the political infiltration of an epistemic community into governing institutions which lays the groundwork for a broader acceptance of the community's beliefs and ideas about the proper construction of social reality (Haas, 1992: 27).

The epistemic community concept is a useful analytical tool, as it draws attention to the influence that scientifically-orientated communities can have on policy-making. Nonetheless, to be able to adequately address the question of how science acquires authority in the first place, the concept requires some tweaking. Most problematic is that Haas' reading tends to under-estimate the influence of governing institutions on the production of the knowledge wielded by epistemic communities. Rather than paying attention to the creation of knowledge or consensus, epistemic theory veers towards treating knowledge as "objective". It too narrowly focuses on the influence knowledge has on policy-making (e.g. Demeritt, 2001, Miller, 2004), neglecting more salient questions such as how and why epistemic communities coalesce and how these groupings achieve their "cognitive authority in the political domain (Jasanoff, 1996b: 187)".

As a result, "traditional" epistemic communities research has been hindered, Jasanoff (1996b) argues, by seven ambiguities relating to both the treatment of the term "epistemic" by political scientists and by theoretical concerns raised by science studies. Significantly, these ambiguities consist of factors both "internal" and "external" to the communities themselves. Addressing these ambiguities, she suggests, helps to overcome the "analytical deficiencies (Ibid.: 186)" inherent in epistemic communities theory. To begin with "internal" factors,[2] Jasanoff points out the need to problematise the ways in which scientific discoveries happen, instead of simply taking this as a starting point for analysis. The ways in which a "shared factual framework (Ibid.: 188)" come into being thus need to be interrogated. Similarly, the origins of the shared worldviews that are said to be held by epistemic communities are, Jasanoff argues, best understood as the result of social processes. Disciplinary interests, in particular, may be influential. She urges analysts to shift their attention to the underlying factors driving the formation and stabilisation of epistemic communities, such as the ways in which disciplinary training as well as professional interests can lead to a shared policy project which "furthers their disciplinary standing and identity (Ibid.: 189-90)". As we have seen in Chapter 1, where influential epistemic communities consist of scientists, scientific knowledge is privileged at the expense of other relevant types of knowledge. It is possible, then, to view an epistemic community's shared values as "nothing more than the overriding interest of scientists in enlarging the influence of science (Ibid*.:* 191)". Analysts that do not problematise the notion of "shared values" may be missing important insights.

Consistent with the emphasis that Chapter 1 placed on interactions between scientists and wider political contexts (and foreshadowing the upcoming discussion on co-production (2.4)), Jasanoff notes three ambiguities surrounding epistemic communities theory related to the ways in which "such communities might be constituted or empowered through the action (or inaction) of the state… (Ibid.: 191)".[3] Jasanoff argues for the need to pay

[2] Jasanoff labels the four ambiguities related to internal factors as: shared factual knowledge about nature; shared causal framework (or paradigm); shared disciplinary interests; shared commitment to science.

[3] These are labelled: convergent economic interests of business and government; convergent domestic policy agendas: shared hegemonic discourse.

attention to the possibility of convergence of economic interests between key stakeholders in a policy process. As she argues:

> Instead of arising independently of … the emergence of international accords, epistemic communities may develop out of the same complex constellation of forces that propel different state actors to adopt similar policy agendas (Ibid.: 192).

If epistemic communities exist not simply within the protected boundaries of science but also in relation to broader socio-economic forces, then the possibility should not be discounted that certain epistemic communities could be "merely the instruments of a technological culture through which powerful industrial states impose a particular vision … on the rest of the world (Ibid.: 194)". After all:

> Sharers in a common scientific worldview are more likely to perpetuate than deeply challenge the political structures to which they are tied by bonds of reciprocal legitimation (Ibid.).

The key point in Jasanoff's analysis is that neither "science" nor "epistemic communities" exist "naturally". Both are the product of social negotiations – internally as well as across broader spheres of influence – and much analytical power is gained by acknowledging this. It could be said that one implication is that considering the science and politics embedded in (and represented by) an epistemic community means treating "science-in-the-making" (Latour, 1987), "community-in-the-making" and "policy-in-the-making" as though they are effectively inseparable. The epistemic community concept, perceived in this way, offers a useful analytical focus for studying the community of experts that populate the climate change and health (CCH) domain. It points to the questions that will be the focus of Chapter 3: how was the knowledge sustaining the CCH community produced, and how did the community's normative commitments influence the production of this knowledge? How did the CCH community manage its political infiltration into governing institutions, and which "external" factors were necessary for this to happen?

To summarise this discussion, the literature on governance is important to this thesis but its treatment of scientific knowledge is somewhat lacking, at least from an STS perspective. As Irwin (2008: 584) has argued: "STS perspectives on scientific governance open up the very definition of such categories as 'science' and 'policy' to critical reflection and empirical reflection". Perhaps none have done this more so than the concept of co-production, which will be more formally introduced in 2.4. Beforehand, it is instructive to briefly explore contemporary accounts of the global health governance landscape, an important "external" factor influencing the activities of the CCH community.

2.3 Global health governance

In the previous section the normative and especially analytical dimensions of governance were discussed. Here, the intention is to describe the rather more empirical accounts that have surrounded *global health governance* (henceforth GHG). As we have seen in 1.3, there has been the tendency to view EIDs as part of the rationale behind strengthening global health security. This has influenced the range of governance options pursued, such as the 2005 revision of the International Health Regulations (IHR), which mandates WHO Member States to strengthen disease surveillance and also gives WHO powers to determine and declare when an EID event constitutes a *Public Health Emergency of International Concern.*[4] The significance of the revised IHRs is such that some have claimed them to be "unprecedented in the history of the relationship between international law and public health (Fidler and Gostin, 2006: 93)". Others noted that the new IHR were indicative of the higher profile that health had achieved in global politics and diplomacy (Horton, 2007).

[4] Such determinations are meant to be evidence-based but also can be expected to occur under situations of scientific uncertainty and high political stakes, raising numerous questions from an STS perspective (e.g. Suk, 2007).

Regardless of the interpretation, it is clear that the increased status of EIDs has influenced the global public health landscape. As it has been argued:

> broader human security concerns about global health … prompted an explosion in the number of new actors, initiatives and funders involved in the field of global health (Elbe, 2010: 115).

One consequence has been a further fracturing of a global health governance landscape that had already changed dramatically over the past few decades. The WHO had held a key leadership position in world health until around the 1980's, when post-Cold War geopolitical trends influenced its role:

> …the UN [health] system seemed to be eroding under the pressure of neoliberal ideologies, an expanding and increasingly concentrated global economy, the growing influence of NGOs, global corporations, new philanthropists, and the noncooperation of the one remaining hegemonic power, the United States (Kickbusch and Buse, 2001: 715).

In addition, globalization is often cited in as having had a formative role. The increased movement of people, goods, services and capital associated with it has been argued to render "national borders irrelevant (Lee, 2004: 157)", diminishing the ability of governments to govern events within their territories. Thus, it has been suggested, States alone "cannot address many of the health challenges arising. Infectious diseases are perhaps the most prominent example of this diminishing capacity… (Dodgson et al., 2002: 8)".

In a global health landscape notable for its absence of any dominant actor, there has been concern about "overlapping mandates, competition and duplication of health activities, poor coordination, and more recently, about issues of governance (Walt, 2001: 691)". Partially for these reasons, specialists within the fields of public health policy, many with ties with WHO,[5] have both observed and called for a transition from *international* health governance to *global* health

[5] For example, Kelley Lee, based at the London School of Hygiene and Tropical Medicine, is Chair of the WHO Scientific Resource Group on Globalization, Trade and Health; Nick Drager is an employee of the WHO; Ilona Kickbusch is a former WHO employee.

governance (Dodgson et al., 2002, Fidler, 2004a, Kickbusch and Buse, 2001, Lee, 2009). These authors, who contribute to what I suggest is the "mainstream" of the global health governance literature,[6] have sought to simultaneously describe the phenomenon, establish a programme of research and map out an agenda for *improvement*. As concerns the latter, challenges that GHG *needs* to overcome are often put forward (e.g. Gostin and Mok, 2009), such as establishing leadership, attracting significant resources, co-ordinating activities across a wide range of actors, developing a commonly-upheld normative framework and convincing states of the need to "pool their sovereignty" and the need to give more "teeth" to global health initiatives (Dogdson et al. 2002: 21) such as, notably, disease surveillance (e.g. Kickbusch, 2000). By 2012, enhancing GHG continues to be a high priority area within public health circles, notably in Europe, evidenced by, for example, the WHO Regional Office for Europe report *Governance for Health in the 21st Century* (Kickbusch and Gleicher, 2012), and a focus on global health governance at the high-level 2012 European Health Forum Gastein meeting in Austria.[7]

Despite such activity, in the past couple of years global health's gain to prominence has been stalled. Rather than trumpeting global public health's rise and "revolution" (Fidler, 2009), the GHG literature has most recently fretted about the diminished role of global health in broader political discussions. Fidler (2009: 15), for example, worries that if global attention to these issues intensifies, it could further weaken the status of global health:

> As global health moves from the headiness of its political revolution to fighting rear-guard actions against global ecological and economic crises, the center of gravity for political, diplomatic, and governance activity for global health shifts from the health sector to political, economic, and environmental contexts in which health policy's voice remains weak because the health sector does not necessarily have persuasive input into how climate change, energy, food, and economic crises should be prevented in the future.

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[6] It should be stated at this point that although I will henceforth refer to it as the "GHG literature", there is no definitive or clear-cut boundary of researchers working from a GHG perspective (e.g. Freeman, 2008): any groupings are my own constructions.
[7] http://www.ehfg.org/home.html, accessed October 3, 2012.

Such concerns have caused actors in the health sector to work to reassert a role for health in such discussions. Margaret Chan, former WHO Director-General, noted the following in her remarks to the United Nations General Assembly in 2008:

> We face a fuel crisis, a food crisis, a severe financial crisis, and a climate that has begun to change in ominous ways. All of these crises have global causes and global consequences. All have profound, and profoundly unfair, consequences for health. Let me be very clear at the start. The health sector had no say when the policies responsible for these crises were made. But health bears the brunt.[8]

Similarly, GHG writers have tended to emphasise the need for global health to better articulate and even re-orientate current policy discussions surrounding these crises to become more focused on health (e.g. Gostin and Mok, 2009, Fidler, 2009, Lee, 2010, Schrecker, 2012). It has been earlier noted (above and in 1.3) that the global health community was generally successful in linking the outbreak narrative to ongoing security discussions, thereby solidifying political commitment to certain forms of EID control, and this has served as inspiration. Thus, some have argued that global health should learn from that experience to "piggyback on other priority agendas (Lee, 2010: 8)" in order to re-direct resources and attention back upon itself. Highly significant to the focus of this thesis, global climate change is highlighted as one of these priority agendas. One implication, to be further explored in Chapter 3, is that global health actors, notably WHO, have contemplated the re-framing of climate change from the tactical standpoint of hoping to re-gain some authority in a fractured public health landscape.

Thus far, the discussion on the "mainstream" GHG literature reveals it to be a rather more normative and empirical than analytical project. As a result, much of the GHG literature, similar to the literature on governance more generally, tends not to problematise expertise and the role of scientific knowledge in

[8] Transcript at: http://www.who.int/dg/speeches/2008/20081024/en/index.html, accessed June 8, 2012. This quote is cited in Fidler (2009).

shaping GHG priorities as well as outcomes.[9] To be fair, these shortcomings have in some cases been partially addressed (e.g. Shiffman, 2002),[10] and indeed some prominent health governance scholars have conceded that the literature tends to be somewhat naïve:

> For students of politics, the link between policy and power is hardly a revelation. In international health, where science and evidence-based practice are keenly upheld, analysis of the role of power in influencing what policies gain and lose currency remains neglected (Buse et al., 2002: 256).

This is a striking omission given that their accounts suggest that power has become dispersed in the contemporary global health landscape. It is furthermore conspicuous that these authors tend to neglect that power might be closely linked to the scientific and evidence-based practice that is so "keenly upheld". Nonetheless, the empirical and normative accounts of GHG provide some important background to the dynamics shaping the global health landscape, which is relevant to this thesis. From an analytical perspective, however, there appears to be much scope for STS research to contribute to a deeper understanding of the processes driving GHG. With this in mind, it is time to discuss the theoretical (2.4), epistemological (2.5) and methodological (2.6) aspects related to studying the co-production of climate change and health as a global risk.

2.4 The co-production of science and politics

2.4.1 Introduction

It has been argued that STS can and should seek to contribute to the broader political science literature (e.g. Jasanoff and Wynne, 1998, Lidskog and

[9] In a recent example, one study describes the epistemic community that mobilised around tobacco control, but it accepts the standard theorisation about epistemic communities at face-value and does not investigate the ways in which earlier scientific controversies got resolved and transformed into a consensual science that all members of the epistemic community shared (Mamudu et al., 2011).

[10] Additionally, as the discussion from 1.3 demonstrated, there are plenty of accounts of global health from outside the "mainstream" GHG literature that are rather more critical in nature (e.g. King, 2002, Dry, 2010).

Sundqvist, 2002, Murphy et al., 2006), given that the "scientific" and "political" aspects of a given topic tend to be both inter-related and mutually shaping. This is a message that appears to have gained traction. As a recent essay about the debates surrounding climate change and their impact on environmental politics concluded: "Political analysis of environmental science needs to consider how science and politics evolve together, rather than identify one or the other as dysfunctional (Forsyth, 2012: 22)".

Thus far, I have argued that a co-productionist STS perspective can contribute to examinations of how and why global risks are framed (1.2, 1.3, 1.4), how governance systems emerge to contain them (1.3, 1.4, 2.3), and how communities of actors mobilise within governance systems (2.2), but I have yet to elaborate upon *what* co-production means, from theoretical and methodological standpoints.

2.4.2 STS and politics

An important preface to a discussion on *co-production* is that there has always been significant disagreement within STS on what the social-political world looks like, how it should be analysed and even whether it is in any meaningful sense separable from the scientific world. This is, in turn, related to the longstanding and widely discussed internal divisions within STS, most notably between the sociology of scientific knowledge (SSK) (e.g. Barnes et al., 1996, Collins, 1983) and actor-network theory (ANT) (e.g. Callon and Law, 1982, Latour, 1988). Despite such divisions, however, one thing that most members of the STS community would agree on is that it is an inherently political subject matter:

> Science is not politics. It is politics by other means (Latour, 1988: 229).

> In seeking to explicate the nature of science and the sources of its authority, *all* SSK scholars are necessarily engaged in an enterprise that is as deeply political as it is intellectual…How could a branch of enquiry that takes as its central

> preserve the making (and unmaking) of human knowledge be anything but *political* to the core? (Jasanoff, 1996a: 409, original emphasis).

There are, crudely, two important implications of this insight that relate to STS accounts of politics and policy-making. One concerns whether analysts should adopt normative stances to take "sides" in the scientific controversies that they are studying. This "neutrality versus commitment" debate, for the purposes of this thesis, is as much a reflexive methodological question as a theoretical one (even if these influence one another), and it will be dealt with in 2.5 and 2.6 where I outline the epistemological and methodological considerations related to this thesis. The other implication relates to how STS accounts for science in governance and policy-making, which will be the focus of the remainder of this discussion. Before proceeding, however, a further division needs to be made. It could be said that there are, broadly, two principal themes that arise when STS examines interactions between science and governance. One of these, already discussed in 1.2, is expertise and its pluralisation in scientific governance, which has led to a vast amount of STS research into risk, regulation, the public understanding of science and the role of public engagement in scientific decision-making. The other, which I suggest is comparatively understudied, relates to how the STS literature specifically addresses and conceptualises science governance systems, institutions and processes. To provide an example of the debates that have occurred when STS has attempted to analyse this topic, it is helpful to discuss one particular example at length.

2.4.3 The science policy construction debate

In 1990, Cambrosio, Limoges and Pronovost (henceforth CLP) argued that science policy analysts continued to treat science policy as a "black box", despite the emergence of STS. Similarly, sociologists of science, they claimed, "have tended either to neglect issues of science policy when analyzing scientific practice, or to tackle them as a separate domain (Cambrosio et al., 1990: 196)". To address this, the authors drew upon ANT and Latour (1987) in particular in arguing that science policy-making consists of representational practices such as the construction and distribution of policy *dossiers,* which they defined as sets of "intragovernmental tasks and constraints (Cambrosio et al., 1990: 200)*".* Dossiers:

> …travel along a path that will lead them to become (or fail to become) a policy. Along this path they undergo a series of modifications, which may completely alter their content (Ibid.: 206).

In CLP's case study, an important element of the dossier was the definition of the practice of biotechnology and its classification into sub-categories. The process of establishing a matrix to represent the various categories of biotechnology, such as dividing biomedical biotechnology between health care and cancer research was highly contingent – but these categories gradually became viewed of as accurate representations of the field. Subsequent consultations with actors from within and outside government helped the policy officials not only to collect data but also to "naturalize" these categories, although in practice the range of "external" actors consulted was quite limited. CLP suggest that the policy researchers selected a limited number of spokespeople to serve as "typical" representatives of certain groups, thereby reinforcing their own original classifications of these groups: "what is at stake in the definition of spokespersons is the construction of what counts as a relevant group (Ibid.: 214)".

In CLP's account, then, the enrolment of selected external actors was helpful for providing policy-makers with an operational impetus for action. The remainder of their paper describes the means through which policy dossiers – and their representations of nature – gained further internal credibility via intertextuality, which in this study was viewed as the practice of referencing other internal documents seen to have authority (e.g. policies, legislations, laws, memoranda) in order to convey certain messages to certain internal decision-makers. Contingent representational practices are thus further stabilized, and to such an extent that "…decisions essentially depend upon internal governmental representations (Ibid.: 219)".

CLP (1990) is among the first STS studies to consider classification-making in a science-policy context, and it led to an interesting discussion which Abraham (1994) labelled the *Science Policy Construction Debate*. Effectively, this debate demonstrates the considerable difficulties that STS practitioners have faced when trying to account for science in policy-making by using the then "established" STS frameworks. As Wynne (1992: 595) noted at the time, this debate "can be seen as another rehash of the debate between discourse-focused and interests-based explanations". Mirroring the broader STS controversy surrounding actor-network theory, for example, CLP were accused by Kleinman (1991) of offering an account that neglected the role of politics, power and social struggle in the production of the various categories of biotechnology. Kleinman further suggested that CLP neglected to explain how various actors' interests became enrolled in the first place; that CLP neglected accounts of important actors like the biotechnology industry; and that CLP generally failed to pay sufficient attention to institutional contexts. In light of all this, Kleinman's recommendation was nothing less than to abandon STS altogether (!): "While we owe a tremendous debt to recent work in the sociology of science, the shortcomings of such work make it clear that it is time to move beyond it (1991: 771)".

In reply, CLP drew upon "traditional" critiques of interest theory to argue that Kleinman: took for granted that "sides" exist; would have started his analysis with "already given, delineated and unquestioned macro-entities (Cambrosio et al., 1991: 777)"; and, for good measure, was not even interested in "what goes on within the black box of policy-making (Ibid.: 778)". Played out in this manner, the science policy construction debate seemed to point to an undesirable end-game for STS: would all attempts at applying various STS frameworks to studies of policy-making simply mirror the more substantial theoretical debates that have occurred in the field?

Fortunately, the interventions of Wynne (1992) and Abraham (1994) offered suggestions that can be seen as reconciliatory. Abraham (1994) critiques CLP's (and also Woolgar's) depiction of the interests approach as static, suggesting that if applied flexibly it can accommodate descriptions of how actors' interests are constructed. He also reconciled some of Kleinman's and CLP's arguments in suggesting that attention to the representations of policies is an important nexus for research, as are the institutional contexts that Kleinman pointed to. Both of these approaches could facilitate analyses of the role of scientists in the political world: "if read constructively, Kleinman's proposals can be seen as an agenda (albeit crude) for sociologists to open up the black box of government science and the institution of science policy (Abraham, 1994: 130)".

Wynne, meanwhile, sides with CLP in stressing that the representations approach is particularly useful for examining "the unspoken contingency and negotiability behind the natural languages of identity and interests which actors carry and deploy (Wynne, 1992: 578)". Yet Wynne is somewhat more nuanced in his endorsement of CLP, pointing out that CLP could have drawn more attention to the negotiable interests of the actors they studied. He suggests that this was perhaps a result of following Latour too closely:

> Latour's work does not always fully recognize nor exploit the endemic tendency to instability…which is introduced via the continually negotiated… and chronically incomplete character of actor's identities (Wynne, 1992: 578).

Together, then, Wynne and Abraham's contributions fall only slightly short of suggesting that there is some room for reconciliation between CLP and Kleinman – or between ANT and the interests approach for STS engagement of the political world. There is also, it would seem, a need to transcend these two approaches. Reflecting more broadly on the application of "traditional" STS frameworks to study "politics", Radder (1998: 330) argued:

> …if we wish to … develop an integrated politics of STS, we must go beyond the traditional actor-network and SSK approaches.

In order to do so, Radder endorsed the "critical and programmatic claims (Ibid.: 328)" of co-production as a means for achieving this – a sentiment that, as suggested earlier, has gained traction in recent years.

2.4.4 The idiom of co-production and the organisation of this thesis

The co-production framework (e.g. Jasanoff, 2004c, Jasanoff and Wynne, 1998, Wynne, 1996b) is perhaps the only STS-derived theoretical framework with the explicit aim of connecting STS to the broader political science literature concerned with the role of science in policy-making. It is arguably not so much a theory as an *idiom* that has been developed to draw upon and bridge a broad range of STS findings (Jasanoff, 2004a). According to Jasanoff & Wynne (1998: 16), an over-arching finding of the STS literature is in fact the idea of co-production:

> Together, this body of work calls attention to the fact that social and cultural commitments are built into every phase of knowledge production and consequent social action, even though enormously effective steps are often taken to eliminate the traces of the social from the scientific world.

One of the main aspects of co-production is that it strives to avoid giving explanatory priority to either the "social" or the "natural". Both are mutually shaping and thus analysts must take care to avoid both social and natural realism. In this regard, it is significant that co-production represents an explicit rhetorical manoeuvre away from *social constructivism*. As Jasanoff argues, labelling scientific knowledge as socially constructed is somewhat misrepresentative of the STS literature. Although constructivist research does not in theory prioritise the social over the natural, the term "social construction" implies this and consequently may "inhibit the symmetrical probing of the constitutive elements of both society and science (Jasanoff, 2004b: 19-20)". Thus:

> …the production of order in nature and society has to be discussed in an idiom that does not, even accidentally and without intent, give primacy to either. The term *co-production* reflects this self-conscious desire to avoid both social and technological determinism in S&TS accounts of the world (Ibid.: 20, original emphasis).

> To refer to 'the science' and 'the policy' as separate domains exchanging independently generated explicit ideas and constraints, is to conceal the *shared* commitments which define them as a single culture (Wynne, 1996b: 377, original emphasis).

In relation to "politics", co-production seeks to analyse "how knowledge-making is incorporated into practices of state-making…and, in reverse, how practices of governance influence the making and use of knowledge (Jasanoff, 2004a: 3)". Additionally, as earlier argued, co-production also represents an attempt to move beyond the sort of STS in-fighting exemplified by the *science policy construction debate* (2.4.4). Underlying this desire is the conviction that STS has something important to say about the links between knowledge and power; that bridging divisions within STS will facilitate its engagement (and, perhaps, uptake) with related literatures; and that such a connection is "urgently needed (Ibid.: 2)" in order to generate more nuanced understandings of how scientists and policy actors collectively produce and mobilize scientific knowledge to both inform and rationalise societal responses to important contemporary issues such as global climate change, EIDs, or, indeed, CCH.

It is noteworthy that co-production incorporates models of scientific governance and policy-making from the broader political science literature into its general framework[11] by more forcefully problematising the status and "objectivity" of scientific knowledge (recall the discussion from 2.2 on epistemic

[11] Jasanoff & Wynne (1998: 7-16), for example, developed their analysis with STS in mind, but they also considered political scientific models of policy-making, such as agenda-setting; knowledge change; policy cultures; and policy interests and discourses. None of these frameworks, they argued, "fully accounts for all the interrelations in the production of scientific knowledge (10)", but they do help to strengthen "the view of the policy-science relationship as complex and nonlinear (10)".

communities theory). Employing co-production to study science and its governance implies treating both symmetrically:

> To question how *science* acquires meaning and stability, by exploring its social commitments, is to question *policy* in the same way (Jasanoff & Wynne, 1998: 5; original emphasis).[12]

Beyond this general insight, the co-production framework usefully draws attention to particular sites for STS research into scientific governance. The remainder of this discussion focuses on some of the central thematic preoccupations relevant to this thesis.

One preoccupation of co-production is *issue framings*, something we have already discussed at length in 1.3 and 1.4 where the risk framings of climate change and of EIDs were examined. The reason that issue framings are a central focal point for co-production research is worth reiterating: knowledge carries with it certain responsibilities and mandates for social and/or political action, and yet it is often also the case that knowledge relevant to public action can be subject to interpretative flexibility. Examining how issue framings originate and how they come to be stabilised can offer much insight into the often tacit or hidden dynamics shaping both "scientific" and "political" worlds, something that will become apparent in Chapters 3 & 4 where the origins and production of CCH research are closely examined.

Applying the insights of STS to studies of *controversy* is another important preoccupation of co-production (as it is for much STS research). One of the reasons for this is the way that studies of controversies help to reveal how

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[12] There is an important ontological ambiguity that should be clarified. The notion of co-production in STS can be traced to Latour (e.g. 1993), who, as pointed out by Bloor (1991), suggests that nature and society are co-produced, rather than society and its *accounts of* nature. From my perspective, it would seem that as Jasanoff and Wynne refer to the co-production of *science* (rather than *nature*), they imply that society and its accounts of nature are co-products. Whether or not this is the correct interpretation, it is how I will understand co-production throughout this thesis.

assumptions and norms are built into scientific research. In Chapter 4, an in-depth examination of a longstanding controversy between the CCH community and a group of disease ecologists will be presented. Analysing this controversy, it will be shown, exposes assumptions built into CCH research that would likely otherwise remain hidden, offering an intriguing counter-point to the mainstream framing of CCH.

Policy-relevant scientific knowledge needs to be understood and transmitted across "time, place and institutional contexts (Jasanoff, 2004a: 5)". Focusing on how science and technology becomes standardised, switches between local and global settings, and even leads to the creation of new institutional settings is yet another preoccupation of co-production. Here, the role of formal scientific governance institutions can be crucial. To explore these themes in relation to CCH science, Chapter 5 will examine the ways in which a particular understanding of CCH science has emerged through the IPCC, an example, *par excellence*, of a boundary organisation, mediating between "scientific" and "political" worlds and attempting to secure legitimacy in both.

To summarise, then, the three thematic preoccupations discussed above (which Jasanoff labels as emergence and stabilization; controversy; and intelligibility and portability)[13] all help to orientate the focus of this thesis and they all fit under the co-production umbrella:

> …interpretative analyses of the framing of policy problems, the production of scientific claims, the standardization of science and technology, and the international diffusion of facts and artefacts all focus attention on the co-production of natural and social order (Jasanoff & Wynne, 1998: 74).

In thus presenting co-production, it can be thought of as a useful heuristic for connecting STS with other literatures interested in policy-making. Numerous STS and other perspectives could in principle be applied to examinations of any

[13] A fourth thematic preoccupation of co-production identified by Jasanoff is the uptake of knowledge and its influences on cultural practices. This not a central focus of this thesis, however.

of co-production's thematic pre-occupations. What is important is that they help elucidate how:

> particular forms of social order or culture, and particular forms of epistemic order mutually reinforce, construct, and validate one another at levels deeper than expressed scientific or policy choice (Jasanoff & Wynne, 1998: 76).

With this in mind, it is time to describe the sorts of epistemological and methodological considerations related to this thesis.

2.5 Epistemology, reflexivity and objectivity

2.5.1 Introduction

Earlier in this thesis I have provided examples of co-production in action, such as the studies of the role of GCMs and anchoring devices in global climate change politics (1.4). It is noteworthy here that Radder (1998) scrutinized Wynne's (1996b) analysis of global climate change. Aside from asserting that Wynne provided a "weak and undogmatic interpretation (Radder, 1998: 329)" of SSK,[14] he asked how Wynne's deconstruction of climate change science would help to answer the question: what to do? If, for example, one is supportive of the need to address global warming, is deconstructing the science (and the politics) surrounding climate change a desirable outcome? Can we, he asks, still act upon the knowledge claims of climate scientists?

The answers one might provide are closely related to their epistemological and methodological perspectives. In this section, I will address some of the epistemological issues that have surrounded STS research and discuss how these relate to the co-production framework (2.5.2). Based on this discussion, in 2.5.3 I will elaborate upon how my methodological choices may have influenced this thesis while also explaining the *political* ambitions of this

[14] Although Radder branded Wynne's usage of SSK as "weak and undogmatic", there is little reason to believe that a strong and dogmatic approach is any better at addressing science in policy.

research. Thereafter, in 2.6 I will describe in more practical terms the ways in which the research underpinning Chapters 3, 4 and 5 was carried out.

2.5.2 Reflexivity and epistemology in STS research

Reflexivity was one of the key tenets of the Strong Programme as outlined by David Bloor (1976). This programme nonetheless came under attack for not being adequately reflexive about its explanations for how interests were constituted (e.g. Callon and Law, 1982, Woolgar, 1981). A selective epistemology was argued to be in play: interests researchers were relativists when it came to scientist's accounts but realists when it came to their own explanations (e.g. Woolgar, 1981). Because so many STS accounts were reliant upon scientific texts, it was said that research from places like Edinburgh tended to "reproduce the language of realism (Lynch, 1982: 242)" rather than paying adequate attention to the discursive strategies used by scientists to produce "reality".[15]

To address this conundrum, an increased emphasis on the ways in which texts, language and practices all help to construct reality was proposed. This so-called linguistic turn paid great attention to the analyst's role in producing "reality", arguing that STS research offers simply another representation of a given phenomenon and that its authors should seek to make this explicit. There was, in other words, the need to "problematize the assumption that the analyst (author, self) stands in a disengaged relationship to the world (subjects, objects, scientists, things)(Woolgar, 1992: 334)".

Pursuing this direction did not, however, prove to be a particularly productive avenue for STS research. Although the reflexive turn did help to draw attention to the performative aspect of texts, which are indeed often primary sources of evidence (as they are in this thesis, see 2.5.4), it is not clear that the additional

[15] As cited by Myers (1990: 26). Myers (1990: 25-34) provides a good overview of several aspects of this debate, which is only briefly summarised here.

layers of irony that often accompanied the desire to make explicit the role of the author in producing accounts led to any particularly useful insights:

> …the reflexivist proposal to turn literary has the odd consequence of transforming all texts into "epistemological" exercises. What a loss of content! (Knuuttila, 2002)

In addition, one important criticism of the reflexive turn came from Lynch (1982), who pointed out the limitation of focusing on scientific rhetoric. Myers (1990: 31) summarised Lynch's argument thus:

> The claim that scientists are using rhetoric is only interesting as an ironic debunking of the assumption that their discourse is especially "objective". Once one grants that this objectivity is something they create in their work, the claim that everything is rhetoric has little meaning.

Lynch (1982) furthermore argued that by focusing too heavily on texts, analysts were becoming too distant from scientific practice. By continuing to view science from "outside" and by reducing science to "inscriptions", richer descriptions of how scientists construct their own worlds, practices, and knowledge were lost. Instead, Lynch argued, analyses should be *situated* within scientific practice; by observing scientific processes in action, analysts can avoid imposing any pre-conceived sociological frameworks. Nevertheless, the programme proposed by Lynch could be said to be equally dogmatic as the one pursued by the reflexivists, requiring instead of intense attention to discourse and reflexivity a high degree of technical knowledge and infiltration into the scientific world.

The reason for describing such approaches here is not to argue for the supremacy of one over the other but to make a few more general points. First, the reflexive turn drew greater attention to texts, which remains an important contribution to the field. Second, reflexivity requires analysts to be more honest about their findings and more explicit about their role in constructing a particular "reality" (to be further discussed in 2.5.4). Third, reflexivity, if viewed more broadly, can in fact offer an escape from the potentially endless game of "epistemological chicken" (Collins & Yearley, 1992) that the reflexive turn seemed to lead to. Once it is accepted that *all* research is contingent and situated, one can instead appreciate that all theories and methods have their strengths and weaknesses. It makes more sense to choose theoretical standpoints suitable to a research topic than it does to dogmatically pursue a particular methodology or to argue that one approach is more reflexive and relativistic than another (Ibid.). This can benefit the field as a whole. As David (2005: 173-4) argued in promoting the concept of "reflexive epistemological diversity":

> To the extent that data collected by different means, and within different theoretical frameworks, leads to conflicting representations of science in society, the sociology of science field, as a whole, benefits in two ways. First, through reflexivity comes the recognition that any singular account, using a limited range of methods and research questions, cannot hope to provide a full account of scientific knowledge….However, second, in additional to reflexivity, multiple accounts provide grounds for complementarity.

In practice, this implies acknowledging that different sub-disciplines within STS can offer interesting insights, while at the same time conceding that there are limitations to any of these approaches. This is a helpful perspective on *reflexivity* as it forces the acknowledgement that no one theoretical approach should be thought to have authority over any other one. The *co-production* framework, functioning as a means for incorporating the diverse insights from STS into a workable framework, is compatible with this perspective.

2.5.3 Neutrality and symmetry

My stance on reflexivity, as discussed above, does not obviate the need to come clean on how my particular standpoints and commitments may have influenced this thesis. Given that the myth of the neutral social observer has been debunked for at least fifty years (e.g. Gouldner, 1962), it would seem appropriate to begin this discussion not by examining whether I have any biases but by clarifying what they are. As I mentioned in 1.2, I attended the WHO's

Madrid Consultation on behalf of my employer,[16] which is the European Centre for Disease Prevention and Control (ECDC), a technical agency of the European Commission with the mandate to "identify, assess, and communicate current and emerging threats to human health posed by infectious diseases".[17]

The fact that my employer has such a mandate would seem to imply that I am a subscriber to the "outbreak narrative" or "EID worldview" that was described in 1.3. As much of my academic and professional life has revolved around EIDs, this is an accusation that would be difficult for me to deny. The best I can probably do is to put it into context.

My undergraduate degree was in microbiology and immunology, and thereafter I spent several years working in the biotechnology industry, primarily for a firm focused on developing antibodies and vaccines against infectious diseases.[18] This industry stands to profit from the EID worldview, and my role was on the business side of things. However, for me personally, working in industry simply alerted me to the many economic, technical and geopolitical barriers blocking the achievement of "global health". One consequence was that I later enrolled in a MSc. in STS, whence I became increasingly interested in the links between the science and politics of "global health". My dissertation looked at the SARS outbreak of 2003. I noted how WHO simultaneously co-ordinated scientific research, issued policy recommendations and negotiated disease control measures with national governments. Each of these activities influenced the other (Suk, 2004), reinforcing, in my mind, the value of viewing science and politics as co-products.

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[16] It is somewhat ironic that my name does not appear in the list of participants at the end of the report summarising this Consultation (WHO, 2008). I am not sure if this is a lucky turn of events helping me to "conceal" my biases, a slight to my organisation (ECDC and WHO have, to some degree, overlapping mandates in the European region), or, most likely, simply a last-minute oversight as I was attending on behalf of a senior colleague who could not attend.

[17] http://www.ecdc.europa.eu/en/aboutus/Mission/Pages/Mission.aspx, accessed June 30, 2012.

[18] http://crucell.com/, accessed June 30, 2012.

Just after I began working on my PhD, I was offered a job at ECDC. This position has given me the opportunity to be actively involved in "global health" while also creating the possibility to work alongside the types of actors – public health professionals and scientists – that I had hoped to gain access to in order to conduct a detailed study of the role of science and politics in global health governance. Through this employment I have also been involved in the production of knowledge about CCH, as well as other types of research that, it could be argued, contribute to the further entrenchment of the EID worldview. It is therefore important to describe how my employment at ECDC relates to some of the key themes of this thesis, before discussing what this means for my ability to produce research that is "neutral" and "symmetrical".

The EID worldview

One of my first projects at ECDC focused on identifying which infectious diseases would pose the greatest threat to the EU in the future. This led to a publication entitled "Future infectious disease threats to Europe" (Suk and Semenza, 2011), that outlines the import of various EID "drivers" often highlighted in the narratives surrounding EIDs, such as climate change, global trade and travel, and demographic change. Based on these, several "threat scenarios" were identified, including, notably, shifts in the transmission of vector-borne diseases.

The very fact that I have produced such an article means that I have contributed to the production of the EID worldview. Yet, as the focus of this thesis should demonstrate, I am also interested in how the risks and policies related to infectious disease are being shaped: I certainly do not subscribe to the EID worldview blindly or unconditionally. I should furthermore note that I am not a big subscriber to the idea of an "imminent plague" and I have problems with the dominant framing of the EID threat. In my work at ECDC I have sought to draw attention to the ways in which social justice and inequalities are linked to infectious disease spread (Suk et al., 2009, Semenza et al., 2010) and I would rather see increased emphasis on disease control and public health in the places that need it the most, rather than measures such as *better* surveillance in richer countries (which I maintain, in disagreement with many of my colleagues, is an over-emphasised public health measure). As I concluded in the paper on "future threats":

> … in an increasingly interconnected world, it would be both ethically responsible and in the self-interest of the richer regions of the world, including Europe, to improve health everywhere (Suk & Semenza, 2011: 2077).

Thus, although I cannot be said to exist outside of the EID worldview, my work at ECDC has attempted to slightly re-position its orientation. This standpoint should lend some comfort to anybody doubting whether I am able to critically assess the role of science related to EIDs in global health governance. To further support this claim, it should also be noted here that I have even bigger problems with the securitisation of health, which as we have seen in 1.3 often connects to the EID worldview. Although my work at ECDC has periodically focused on "biosecurity" and "bioterrorism", I have diverged paths with many who share the EID worldview to explicitly argue that the risks from bioterrorism appear to me to be exaggerated. In more technical terms, I have argued that the great attention that has been placed on potentially "high tech" bioterrorism threats (such as a pathogen engineered so as to be more dangerous) appears to be misguided, particularly when contrasted with the numerous diseases that have "naturally" emerged in past decades (Suk et al., 2011). As I see it, any potential threat (and threat mitigation measures) related to bioterrorism must be placed in a context that also considers the day-to-day impacts that infectious diseases have on many populations of the world.

CCH Research

As CCH research combines narratives surrounding EIDs as well as climate change, and as it is of central interest to this thesis, my relationship to two aspects of CCH research – the content and its producers – needs to be clarified. To start with the content, it must be said from the onset that, from my personal as well as professional standpoint, I "believe" in climate change. I am deeply aware of the uncertainties inherent in this complicated field, but generally feel that even if only some of the consequences predicted by the dominant narratives surrounding climate change could materialise, then climate change is well worth avoiding, if at all possible.

With regards to CCH research and the potential impacts of climate change on infectious diseases in particular, I am more ambivalent. However, governmental work in the EU tends to assume that climate change *will* have some sort of impact on disease transmission, and this is the general spirit with which my work on climate change at ECDC has been undertaken. For example, I have coordinated a project with the aim of producing a manual to assist EU Member States to assess their possible vulnerabilities and then develop adaptation strategies to climate change (ECDC, 2010). Similarly, in response to a question about how current disease surveillance systems should be altered to anticipate the impacts of climate change, I ended up co-authoring a short essay, alongside a few prominent members of the CCH community, in the journal *Science*. The message underpinning this essay is that climate change will likely lead to some changes in the distribution patterns of infectious diseases, even if we did concede that "the impacts of climate change on infectious diseases are complex and multifaceted (Lindgren et al., 2012: 418)".

Such complexities are difficult indeed for organisations that must decide whether (and how much) attention should be given to CCH issues. In another paper, we have reflected upon the challenges related to being a public health agency tasked with assessing the impacts of climate change:

> Attributing single infectious disease epidemics to climate change is not possible, but longer-term trends in disease outbreaks and incidence may signal linkages to climate variations. The exact attribution of changes in specific infectious disease risks to climate change is probably not attainable. Nonetheless, public health practitioners are obliged to address credible risks—even if that requires acting in the absence of conclusive evidence (Semenza et al., 2011: 391).

This nicely alludes to some of the principal preoccupations of this thesis. How are risks deemed to be credible? In the absence of conclusive evidence, which experts should be listened to?

That I am asking such questions should indicate that I am not completely satisfied with the way in which particular visions of CCH research have become "dominant". And yet although the quote above should also help to exonerate me from any claims that I am completely biased when it comes to CCH research, the very fact that I have published and worked in this area would seem to indicate that I, at least in my "professional" capacity, have an interest in treating climate change as an important health threat.

Here, I should point out that, perhaps ironically, my professional exposure to CCH issues has in fact led me to be more cautious about the field as a whole. As I gradually transitioned from one working with CCH knowledge for public health purposes to one more intimately involved in the production of this knowledge, my level of uncertainty has correspondingly increased (mirroring the path of the "certainty trough" (MacKenzie, 1990)). The reason for this is quite straightforward. At ECDC, rather than simply accepting the findings from the CCH community or the IPCC, we eventually started to produce research more directly aimed at clarifying the links between climatic variables and infectious diseases (e.g. Semenza et al., 2012). This has exposed me to many of the technicalities, uncertainties, embedded assumptions, etc., (to be discussed at length in Chapters 3, 4 and 5) that are inherently connected to CCH research, increasing my doubts about the certainty – and in many instances the "credibility" – of the science.

The increased caution with which I currently interpret CCH research is also due to the many interactions that I have had with both CCH proponents and opponents. Although some of the aforementioned articles that I have published at ECDC have been co-authored with prominent members of the CCH community (like Kris Ebi, Elisabet Lindgren, or a senior colleague at ECDC, Jan

Semenza), I have also had professional relationships with opponents of CCH research, most notably Paul Reiter, Sarah Randolph, and David Rogers. I have even been the project manager of a project for which David Rogers was contracted by ECDC to model how climatic variables affect the risk to Europe from dengue (ECDC, 2012). In other instances, I have hosted and participated in numerous meetings at ECDC that became quite heated when both CCH opponents and proponents debated over topics such as whether we should expect more tick-borne encephalitis (TBE) or dengue in Europe due to climate change.

It was, in fact, such interactions that raised my attention to the deep-seated nature of the controversies in CCH research in the first place, probing my interest in getting to know both sides of the story – both for my work at ECDC as well as for this thesis. Thus, today, my "professional" stance on CCH research is quite a bit more sophisticated than it was a few years ago.

On neutrality and symmetry

In terms of its potential impact on this thesis, my employment at ECDC is problematic. I am not a neutral observer of CCH research or of its role in the broader global health political landscape. Thus it is necessary to consider whether (and how) this loss of neutrality may impact this thesis. To begin to examine this question, it is important to note that many STS scholars have noted that neutrality is simply not achievable in practice (Martin, 1996, Scott et al., 1990), perhaps particularly when studying controversies:

> Analysts....are not 'apart' from the controversy they study, but invariably implicate themselves in it. Their arrival on the scene alters the discursive configuration and subtly modifies the object of investigation itself. The effect, intended or unintended, is to overturn a specific hierarchy of plausibilities (Pels, 1996: 294).

This raises the important question of *how* analysts implicate themselves in a controversy – if analysts are not neutral, should they be openly partisan in pursuing their research, particularly when studying issues relevant to public policy? For an issue as "grand" as climate change or CCH, it is certainly tempting to be so:

> Whether solar neutrinos or gravity waves exist … are questions of little significance when compared with those which affect (or potentially affect) us all. In such cases, it is much more difficult for the analyst to distance herself from the debate (Richards, 1996: 339).

There is, however, a difference between acknowledging that it is difficult to remain distant from a debate and by actively taking sides in one. The latter position, as has been observed by several, is problematic both in practice and in theory. Practically, it is not as easy as it seems to identify the winners, losers or the underdogs in a given controversy. Collins (1996), Jasanoff (1996a) and Richards (1996) have all, for example, suggested that their research was "captured" unpredictably by actors from various sides of a controversial issue. Somewhat more theoretically, Wynne (1996b), meanwhile, suggested that the very discussion about choosing sides tends not to have problematised what constitutes a "side" in the first place, thereby reproducing a "deterministic, one-dimensional implicit model of politics and interaction (362)".

To return to this thesis, I will be studying a controversy and, as earlier noted, I cannot claim to be neutral. This does not, however, mean that I will seek to take "sides", presuming that these are easily identified. In addition to the arguments presented above, there is a sound pragmatic reason for this. As I have already mentioned, CCH research is quite uncertain and controversial – there is a "dominant" but not "orthodox" position within the field – and even if I admit that a goal in my "professional" life is to promote the best possible public health response to climate change, it would be quite rash for me to do so by unequivocally choosing to side with either CCH proponents or CCH opponents. What if the "truth" is somewhere in between, or, worse, I choose the wrong side?

There is one further reason why I would not want to choose sides. This research does not aim to achieve some kind of closure for the CCH controversy. Rather, I hope to use the insights from an analysis of this controversy not only to better understand its roots but also to say something broader about how (uncertain and contested) climate change science achieves credibility in global governance processes. This, in turn, consistent with my self-confessed "belief" and in climate change, *might* enable me to contribute to current discussions about what institutions like IPCC might do as to shore-up the credibility of climate change science. Thus, although I cannot deny that I am a *committed* analyst, I do have good reasons to attempt to conduct a balanced analysis.

Some STS scholars have asserted that it is perfectly reasonable to "reject neutrality, but still be symmetrical (Pinch, 1993: 371)". Just as "neutrality" is impossible to achieve in practice, neither can a symmetrical approach succeed in being apolitical. In trying to conduct my research symmetrically, for example, I may well end up giving more space to the minority or the marginalised than they are accustomed to. This conundrum, it has been pointed out, even exists for "neutralists" (e.g. Collins, 1996):

> In a field of unequally distributed symbolic power or symbolic capital, a symmetrical approach invariably subverts the dominant view, and strengthens the side of the weak and the marginal (Pels, 1996: 282).

Should this happen, it would certainly be ironic, as I could end up strengthening the voice of people whose professional positions tend to be somewhat opposite to my own (I am referring here to those who might oppose not simply CCH research but the idea of climate change more generally). Yet this should not be overly problematic for me, for, as noted above, I do not subscribe wholeheartedly to any one "side" of the CCH controversy. Moreover, another of my objectives is to use the insights from the controversy to reflect on the credibility-making of climate science more generally.

No matter what I say, however, it must be stressed that all normative or theoretical objectives in this research are subsidiary to the ultimate objective of ensuring that my analysis is as balanced and robust as possible. As Richards (1996: 347-8) reminds us, STS researchers should:

> … try to ensure that our analyses are as comprehensive as we can make them, and that they take account of (and give voice to) the marginalized and disempowered. Our specificity as intellectuals studying scientific and technical 'truths' and the systems of power that produce and sustain them … gives us a unique and responsible relation to and knowledge of one of the central 'regimes of truth' of our society. But this knowledge … does not make us the bearers of universal truth and justice.

In this thesis, I will be providing *my* account of the topic, which I cannot even guarantee will be used by others in ways consistent with *my* objectives, filtered as it will be through multiple (and potentially contradictory) professional and intellectual commitments.

2.6 Research strategy and design

2.6.1 Ethics

Before discussing the research strategy and design, it is important to address the ethics of this research, particularly given that I have conducted this research while under full-time employment. As discussed at-length in 2.5.3, although I cannot claim to be "neutral", neither can any honest researcher. As it was further noted, that I am a *committed* researcher is not necessarily problematic from the point of view of STS. As concerns the ethics of conducting this research while employed at ECDC, it is important to stress that ECDC, was made aware of this research during my first interview for this post, and has never expressed any concerns about it. Indeed, ECDC has been generally supportive of this project, even enabling me to take the odd period of unpaid leave to work on this thesis.

As relates to those that I have interviewed (2.6.4), all were aware that the interviews were related to this PhD research and not to my employment at ECDC. The objectives of this thesis were explained to all interviewees. It is important to note that although I am reporting on various scientists' views (albeit anonymously) about a highly polarised scientific controversy, for the most point the scientists have already openly published their stances to this controversy – this research is focused on clarifying such stances, but will not expose stances that the scientists have expressly desired to keep private. Quite the contrary: many interviewees were keen to speak with me to clarify and elaborate upon their perspectives. Moreover, all interviewees were given the opportunity to apply *Chatham House Rules* to entire or portions of their interviews (to be further discussed in 2.6.4). No interviewee expressed any concern about the open publication of their standpoints, and only in a very few instances did interviewees ask me to switch to Chatham House Rules, and in these instances it was only to make the odd statement.

Finally, owing to my employment at ECDC, I have been privy to information that is confidential – draft versions of reports, for example, and private conversations with interviewees or other actors with something interesting and relevant to say. While this has inevitably informed my perspectives, no confidential information is cited or drawn upon in this thesis.

2.6.2 Overview of the research strategy

The research in this thesis was undertaken through an *abductive* research strategy (Blaikie, 2000: 114-119), which is most closely aligned with the interpretivist epistemological orientation of STS and the co-production framework. Just as interpretivist research tends to view individuals and groups as constructing their own versions of reality, an abductive research strategy "seeks to discover why people do what they do by uncovering the largely tacit, mutual knowledge, the symbolic meanings, motives and rules, which prove the orientations of their actions (Ibid.: 115)". This approach has been linked to the more ethnographic approaches of STS. Drawing on some of Latour's (1987) arguments, Becker (1996) argues that an abductive strategy (although he does not label it as such) is required in order to see beyond the formalised accounts of actors:

> Epistemologically…qualitative methods insist that we should not invent the viewpoint of the actor, and should only attribute to actors ideas about the world they actually hold, if we want to understand their actions, reasons, and motives.

In the undertaking of this research, I have sought to obtain the viewpoints of actors by observing them in action; seeking out the materials that they have produced; and speaking with them directly. Analytically, my primary objective has been to compare and contrast different standpoints between actors – necessitating not "neutrality" but "symmetry" (2.5.3), through which I have tried as much as possible to understand the viewpoint of actors on both sides of the debate surrounding CCH. A related objective in this research has been to identify changes in the standpoints of actors and of official bodies: have viewpoints on CCH changed with time, and if so, how and why?

I have drawn upon three separate sources of information in this thesis, summarised in Table 2. These include observation, primary documents, and interviews with key participants. The selection of these data sources has been designed such that they account not only for "scientific" practice but also the activities of scientists in more policy-driven arenas, most notably IPCC. Aside from providing a richer range of information to analyse, drawing upon multiple data sources has the additional benefit of somewhat enhancing the internal validity of this research. This was another important objective of the research strategy because of the potentially compromising influence of my employment at ECDC. For example, to ensure that a participant has not simply said something to me during an interview to appeal to my "professional" role, it has been helpful to corroborate their statements with what they have written in the peer-reviewed literature, stated at conferences, or submitted as revisions to the IPCC. Such a strategy, of course, also contributes to a "thicker" analysis.

Table 2. Data Sources

Type of data	Stage of research	Source of data	Treatment of data
Observational	Initial 24 months at ECDC (2007-2009)	Informal notes on meetings, conferences, conversations participated in as an employee at ECDC.	Contextual: identifying case studies, gaining deeper understanding of the controversies surrounding CCH, identifying key actors and publications.
Interviews	2008 - 2011	22 semi-structured interviews with scientists, policy-makers and public health actors holding extensive experience in CCH and/or at WHO and/or at IPCC.	Interview data represent the accounts of actors, not absolute "truth". Interview data were coded according to categories that were generated through the collection and analysis of data.
Documentary sources	2008 - 2011	Primary documents (e.g. research papers and editorials in peer-reviewed and specialist journals, books written by key actors relevant to the case study, IPCC assessment reports and documents summarising all submitted review comments). Secondary documents (e.g. previous social scientific analyses)	Texts are understood to be performative: they are examples of scientific "work", not of scientific "truths". Primary sources are analysed with the intention of uncovering hidden assumptions and evidence of social processes. For this reason, differences between texts, whether published disputes over specific research papers, or changes in the standpoints of authors or organisations with time, are of particular interest.

Employing multiple data sources can be thought of as triangulating data, which is generally thought to be a means of protecting against bias and ensuring validity. Analysts from Denzin (1970) onwards have discussed various forms of data triangulation, such as within-method and between-method triangulation. The notion that it is possible to confirm findings through the application of varying vantage points is indeed attractive, but its relevance to abductive research has been challenged:

> With an … epistemology that recognizes that accounts of any social world are relative in time and space, and to the observer, the use of multiple data sources and multiple observers do not do for the *abductive* strategy what it is claimed they do for the inductive or deductive strategies … Triangulation, as originally conceived, is irrelevant to the *abductive* strategy (Blaikie, 2000: 268, original emphasis).

This, Blaikie suggests, is because interpretivist research, by its very nature, is much less concerned with bias or validity as formally understood. Nonetheless, it is my intention to develop a robust and credible analysis, and as I have discussed above, employing multiple data sources and methodologies is a useful means of achieving this, as is the use of multiple case studies.[19] Bearing these points in mind, below I will elaborate upon the different data sources that I have used in this research.

2.6.3 Observational data

My role at ECDC has significantly facilitated this thesis, as it was through ECDC that I gained in-depth exposure to the CCH field and discovered the extent of the controversies surrounding this field, and it was through ECDC that I also observed the increasing political importance of CCH. Strictly speaking, I have not been an "external" observer of CCH activities. I have been, one could say, a deeply entrenched participant observer. I have worked on and commissioned CCH science and deliberated on how to interpret this knowledge from the perspective of a public health agency.

In the course of such activities, and particularly during my first couple of years at ECDC, I developed a comprehensive set of observational data, in the form of informal notes on meetings, conferences and conversations that I have participated in at ECDC. I have only used this data to inform and help refine the focus of this thesis and to identify interviewees and relevant documentary materials to examine. I will not present any of this data in the following chapters. The reason for this is both ethical and practical. It would not have been easy to continually flip back and forth between my ECDC role and my PhD role (this would have compromised both!) and, more importantly, it would not be particularly ethical to present research data based upon my notes that other

[19] Even Blaikie (2000) does admit that triangulation can be compatible with some versions of the abductive approach. His main concerns seem to be that triangulation is widely but inconsistently used and that it encourages a naivety towards ontology and epistemology.

participants were not aware I was taking, or based upon confidential information obtained solely through my role as an ECDC employee.

2.6.4 Interview data

Intending as it does to identify the social and political factors related to CCH science, this thesis is necessarily heavily based upon interview data. The interviewees in this research were selected because they were both accessible to me and relevant to the case studies of Chapters 3, 4, and 5. It is no accident that these case studies are interconnected, with the consequence that many of the interviewees had important perspectives to offer on more than one of the case studies. Especially with early interviewees, I employed the snowball technique, a classic approach to gain hard to reach actors (e.g. Atkinson and Flint, 2002), which helped to identify and gain introductions to relevant interviewees.

Following the central SSK tenet of symmetry, I have tried to consider as much as possible the perspectives from those on both “sides” of debates (2.5.3). In total, I interviewed 22 people, all of whom had intimate knowledge of CCH science and politics, and/or, in some cases, of WHO or IPCC. Collectively, they include high-level scientists from academia, representatives of public health agencies including WHO, Health Canada, the US CDC and ECDC, sister agencies like the European Environment Agency (EEA), political actors from the European Commission, and numerous people who had experience in the IPCC assessment process as authors and reviewers (Table 3). All interviews were conducted privately and under confidentiality, and all interviews were recorded with the permission of the interviewee. Where possible, interviews were conducted in person, but in some instances interviews were conducted over telephone or videoconference via Skype™.

Table 3. List of interviewees

Name	Title	Affiliation (at time of interview)
Ingvar Andersson	Scientific Liaison Officer	European Environment Agency
Peter Barry	Senior Policy Analyst, Climate Change and Health	Health Canada
Ian Burton	Professor Emeritus	Department of Geography, University of Toronto
Kristie L. Ebi	Head, Working Group II Technical Support Unit	Intergovernmental Panel on Climate Change (IPCC)
Duane Gubler	Department Chair	John A. Burns School of Medicine, University of Hawaii
Simon Hales	Research Associate Professor	Department of Health, University of Otago
Dorota Jarosinska	Scientific Officer, Environment and Health	European Environment Agency
Marina Koussathana	Seconded National Expert	DG SANCO, European Commission
Sari Kovats	Senior Lecturer in Environmental Epidemiology	Faculty of Public Health and Policy, London School of Hygiene and Tropical Medicine
Marianne Lilleskӧld	Swedish Focal Point for IPCC	Swedish Environmental Protection Agency
Elisabet Lindgren	Environmental Epidemiologist	Institute for Environmental Medicine, Karolinska Institute
George Luber	Associate Director for Climate Change	National Center for Environmental Health, U.S. Centers for Disease Control and Prevention (CDC)
Celie Manuel	Intern	Department of Public Health and Environment, World Health Organization (WHO)
Pim Martens	Professor, Chair, Global Dynamics and Sustainable Development	International Centre for Integrated assessment and Sustainable development (ICIS), Maastricht University
Anthony McMichael	Professor, Population Health	Australian National University
Rainer Melicke	Scientific Officer	DG SANCO, European Commission
Bettina Menne	Programme Manager, Climate Change, Sustainable Development and Green Health Services	European Centre for Environment and Health, WHO Regional Office for Europe
Sarah Randolph	Professor of Parasite Ecology	Department of Zoology, University of Oxford
Paul Reiter	Director, Unit of Insects and Infectious Diseases	Department of Infection and Epidemiology, Institut Pasteur

David Rogers	Professor of Ecology	Department of Zoology, University of Oxford
Jan C. Semenza	Senior Expert	European Centre for Disease Prevention and Control (ECDC)
Alistair Woodward	Professor, Head	School of Population Health, University of Auckland

As mentioned earlier, I gave all interviewees the opportunity to apply the "Chatham House Rule"[20] when they wanted to discuss something particularly sensitive. Yet in terms of writing the thesis, to further safeguard the privacy of interviewees, I have written up the vast majority of the thesis according to the Chatham House Rule. I have thus anonymised nearly all quotes from interviewees cited in this thesis by allocating, randomly, a code to correspond with each interviewee (from R1 to R22). In the few instances where quotes are not anonymised, this is because it will be almost certain to the reader who the speaker actually is, and providing the anonymous code would have then made it possible for a reader to identify that interviewee throughout the thesis. In these instances, none of the quotes I have cited were taken under the Chatham House Rule.

During interviews, semi-structured research questions were tailored to individual interviewees' backgrounds, roles, research interests and institutional affiliations, in order to elicit as much relevant information as possible. Aware of the possibility for varying discursive repertoires among interviewees, such as "empiricist" and "contingent" ones (Gilbert and Mulkay, 1984), I sought to probe the contradictions and tensions inherent in such varying discourses. For example, much of the interview content was focused upon the scientific controversy surrounding CCH research – namely the controversy surrounding whether or not climate change can be expected to impact the transmission of vector-borne diseases. This is a very visible controversy, for which actors on both sides of the debate have very openly expressed their standpoints through

[20] "When a meeting, or part thereof, is held under Chatham House Rule, participants are free to use the information received, but neither the identity nor the affiliation of the speaker(s), nor that of any other participant, may be revealed". See: http://www.chathamhouse.org/about-us/chathamhouserule, accessed July 12, 2012.

peer-review articles, editorials, news stories in the popular and scientific media, and conferences. As noted in 2.6.1, I will not necessarily reveal anything new about which "side" of the controversy interviewees support – this is already widely known. Instead, my analytical attention has been focused on the ways in which interviewees have "accounted for error" by drawing variously upon empiricist and contingent discursive repertoires. As Gilbert & Mulkay (1984: 67) describe this:

> ... the speaker (a) identifies the views of one or more scientists as mistaken and (b) provides some kind of account which enables us to understand why the scientist(s) adopted an incorrect theory or failed to accept a correct theory. Any passage which displays these two features is an example of 'accounting for error'.

Accounts of error in a scientific controversy are highly interesting because scientists, who themselves tend to maintain the empiricist repertoire in relation to their work and even their discipline, must nonetheless draw upon other resources to explain away their opponents' errors:

> … each speaker who formulates his own position in empiricist terms … sets up the following interpretative problem: 'If the natural world speaks so clearly through the respondent in question, how is it that some other scientists come to represent the world inaccurately?' … This implicit question is resolved in accounts of error by the assertion that the views of these other scientists are being distorted by the intrusion of non-scientific … influences into the research domain (Ibid.: 69).

Gilbert & Mulkay note that accounting for error is not restricted in scientists' oral discourse. It may also occur in the literature, notably editorials or historical essays, for example, and it goes without saying that as I analyse documentary data (2.6.5) I will be looking for this phenomenon. It is important to note that there may have been instances in which interviewees' accounts may not have been fully factual or even truthful. I have tried to corroborate facts, and sometimes have left footnotes underneath interview data to clarify factual errors, but it is not always possible to know where an interviewee may have been deliberately deceitful. I have reported interviewees' accounts as they said them as supporting evidence of the arguments that I am making, which, it should be stressed, is different than interpreting interviewees' accounts as evidence of the truth of their statements. Thus, I urge readers of this thesis to remember that there is an element of contingency in interviewees' accounts.

I should finally acknowledge that I have likely played a role in influencing interviewees' accounts. A wide variety of factors have been noted to influence the data obtained from interviewees. These can range from the credibility that one gains with interviewees (Rubin and Rubin, 1995) to the possibility of skewing the interview by letting personal interests interfere (Seidman, 1998). Seemingly mundane things such as presence or absence of tape recorders (Arksey and Knight, 1999) could also impact interviews. In order to prevent, insofar as is possible, any undue bias in my interviews, all efforts were made to avoid leading or double-barrelled questions and to maintain a natural and relaxed interviewing environment (Rubin and Rubin, 1995, Hyman, 2004, Atkinson and Flint, 2002). Perhaps because of my position at ECDC, I was "known" to many interviewees, which I believe enabled me to obtain particularly candid answers. Although it is impossible to be sure, I believe that I had credibility among the interviewees as a sort of informed observer, and I also believe that I was trusted to be relatively "neutral": many interviewees sought to convince me of the merits of their particular standpoints rather than assuming that I already had a strong standpoint from which I would not budge. Finally, as mentioned earlier, all interviewees understood that the interviews were not related to my role at ECDC but were instead related to this thesis.

2.6.5 Documentary data

Texts are among the key sources of evidence available to an STS scholar, and many were analysed in this thesis. This includes editorials and scientific commentaries, research papers, and books penned by relevant actors; official documents such as those produced by the IPCC or WHO; and the odd news story in scientific journals that contain quotes from actors of interest. All of these sorts of documents were treated as "primary".

Primary data was not obtained through any particular keyword search strategy. Instead, I tended to start with research papers and editorials either identified through observation or discussed by interviewees themselves. Particular focus was, naturally enough, paid to sources relevant to the specific case studies. A snowballing approach was also employed, whereby the bibliographies of initial sources, alongside other publications that referenced them, were also assessed. There was no specific date range with which I restricted the inclusion of primary data sources into the analysis. I did, however, privilege sources that had correspondence connected to them (such as letters refuting or supporting the claims of a research paper) and publications in the more prominent biomedical and public health journals, as judged by the *Impact Factor* score of the *Science Citation Index.*[21]

It is necessary to comment that, borrowing from the linguistic turn in STS, I view texts as being *performative* – they are not passive products of scientific knowledge but are part of the process of scientific knowledge construction. Documents can be seen as attempts to establish credibility and create scientific truths: they are evidence of scientific "work" but not of scientific "facts" (e.g. Myers, 1990). This, of course, holds true for policy and "official" documents as much as for "scientific" ones (Freeman and Maybin, 2011). Consistent with researchers interested in the "work" that documents do, my interest has been in trying to get behind the texts to uncover hidden assumptions and evidence of social processes, such as accounting for error (2.6.4). I have moreover attempted to identify the human decisions and activities that are inevitably part of the production of texts (choices that I have sought to further identify and describe through interview data). I have thus been particularly interested in analysing published disputes over specific texts and in identifying changes in

[21] See http://thomsonreuters.com/products_services/science/science_products/a-z/science_citation_index/, accessed July 12, 2012.

the writings of various authors with time. I have furthermore been interested in juxtaposing the editorials that these scientists have written with their research papers. Where it concerns the IPCC, I have been interested in the changes between assessments and, for the IPCC 4th Assessment Report, in changes between draft versions and the final version (all of which are publicly available).

Primary documents have been complemented by secondary sources, such as analyses of topics related to this thesis from the social or political science literature.

2.6.6 Data analysis

This thesis is the outcome of an iterative process involving data collection and analysis. The analysis has evolved as more data has been collected, and the types of data collected have been informed by this evolving analysis. Even the case studies themselves are the product of data collection, for early observational data helped to refine their focus.

As Blaikie (2000: 31) has noted about iterative processes:

> …analysis may be integrated with data collection into a continuous and evolving process of theory construction. This will involve establishing categories and doing various kinds of coding.

Coding in this sense is both a means of filtering and reducing data, as well as a form of analysis. There are a variety of approaches to the coding of data, but they all tend to open some pathways for investigation and foreclose others (Mason, 2002). Following Strauss & Corbin (1998), I employed an open coding approach in this study, whereby the codes and categories of codes directly emerged from the research data, but I did not follow all of the systematic steps related to grounded theory. Instead, following each “wave” of analysis, I refined the codes until I ended up with a clear set of themes.

It is relevant to describe how this was done in practice. Interview transcripts and interesting excerpts of text from primary documentary sources were coded using the HyperResearch™ software. Many excerpts of text had more than one code allocated to them. Initially, I was focused on grouping the data according to very broad "scientific" and "political" themes I was interested in (e.g. CCH at WHO; CCH at IPCC) whilst STS-related themes, such as "controversy" and "boundary work", were employed but kept to a minimum, so that I could focus on the "narrative" structure of the thesis. As more data accumulated and the demarcations between the different case studies that eventually became Chapters 3-5 became clearer, I started to categorise text as belonging to one or more of these case studies (the "rise" of CCH; the climate-vector-borne disease controversy; and CCH in the IPCC assessments). At this point, text was much more heavily coded, with the same coding structure used for all case studies. For example, text was coded according to disease (e.g. dengue or malaria), according to numerous STS-related themes (e.g. consensus, boundary work, expertise, funding, evidence base, standards of proof, policy-engagement, interpretative flexibility, prestige), and according to its context (e.g. controversy in the peer-reviewed literature; specific IPCC assessment reports; official WHO or other reports).

The analyses conducted in this research could be subjected to the sorts of critiques that relate to validity of qualitative research more generally. For example, it has been argued that case studies raise issues of internal and external validity (Stoecker, 1991). To address this, as already noted, I have employed multiple data collection methods and sources (e.g. interviews, document analysis) as a form of triangulation. As concerns the external validity (i.e. generalisability) of my research, I follow those that have argued that theoretical and analytical reasoning are more important than external validity in case study research: "Generalisation is analytical and not statistical…the extent that a case study illuminates and develops theory, we may treat it as representative (Bechofer and Paterson, 2000: 49)". Nonetheless, as we proceed through the following chapters, it should be kept in mind that the arguments that are contained in this thesis must necessarily be understood as *my* arguments, contingent on my professional, academic, and normative interests.

Chapter 3. Epidemic Communities

"Medicine is a social science, and politics is nothing else but medicine on a large scale" – Rudolph Virchow

3.1 Introduction

In Chapter 1, the emergence and stabilisation of two "global" risks, emerging infectious diseases and climate change, was discussed. Alluding to the co-production framework described in Chapter 2, both, it was argued, have created, maintained and reinforced a particular conglomeration of scientific and political actors and interests. This Chapter examines the merger of these two risks by exploring the way in which climate change has been re-framed a public health risk.

This thesis aims to understand the interrelationships between science and global governance and, as it has been argued, one way of doing so is to problematise expertise: who produces policy-relevant knowledge, and how and why? A community of epidemiologists interested in climate change and its impacts on health (henceforth the CCH community) have been particularly influential in raising and maintaining CCH as an issue for global health governance. The CCH community pursued a branch of research that was initially fairly speculative. Future and policy orientated, this work did not fit well within "traditional" epidemiology. Instead, the CCH community drew inspiration from a branch of epidemiology that had adopted an "ecological" approach. From the onset, the CCH community embarked upon an active programme of extra-curricular work in order to secure their field: demarcating and defending their work from "traditional" epidemiologic research; raising awareness among policy and funding communities; gaining traction at WHO and IPCC. It is noteworthy that in the early days of CCH research, WHO and IPCC did not have any *a priori* structures in place to address the topic. By eventually considering CCH, however, these agencies validated it as a worthy topic and thereby paved the way for further funding, research and policy initiatives in this area. Climate change and its existing discourses and policy processes ultimately offered a strategic ally for CCH work: climate change was a "convenient truth" for the CCH community.[1]

This Chapter begins by contextualising the broader relationships between public health and epidemiology (3.2). In 3.3, the key actors in the CCH community are identified as are their attempts to define and broaden the field. CCH eventually gained traction and helped influence reorganisations within international agencies, notably WHO and IPCC. This will be discussed in 3.4 and offers important insights into the manner in which CCH science and politics have been co-produced, with ramifications for how the vast amount of uncertainties inherent in this research, exposed through a longstanding controversy within the field (Chapter 4), would eventually be addressed by the international community (Chapter 5).

3.2 Epidemiology and public health: political engagement and shifting paradigms

3.2.1 Epidemiology, public health and decision-making

The modern birth of epidemiology is often associated with John Snow's famous investigation of a cholera outbreak in London during the 1850's, in which he identified the notorious Broad Street pump as the source of the outbreak at a time when contagion theory was competitive with other paradigms, such as the miasma theory of disease spread (Snow, 1855, Snow, 2008). Today, epidemiology is applied to the study of health outcomes with a wide range of time scales, from the relatively short-term, such as the outbreak of an infectious disease, to the long-term, such as the influence of smoking on the incidence of lung cancer. Regardless of time-scale, one of the key objectives of epidemiological research is to identify risk factors and assess their relationships to various diseases. As such, much epidemiological research involves reaching

[1] Thanks to Steve Yearley for suggesting this pun on Al Gore's film *An Inconvenient Truth*.

statistical conclusions attributing various disease or health outcomes to various disease or health determinants (and vice-versa).

Epidemiology tends to be viewed as the "basic science" of public health (e.g. Gregg, 2002) and indeed the two are nearly inseparable. The WHO defines epidemiology as "the study of the distribution and determinants of health-related states or events (including disease), *and the application of this study to the control of diseases and other health problems* (emphasis added)".[2] Similarly, many universities around the world combine public health and epidemiology into one school or department, and it is unimaginable that a public health course would not insist upon teaching its students epidemiology. At the higher echelons of the field, professors and other notable epidemiologists tend to have great influence on the major public health agencies in the world, whether as high-level administrators, highly paid consultants, or field agents conducting on-the-ground assessments.

Although public health policy-making may be modulated by socio-economic and political circumstances, epidemiology is its privileged knowledge source. Despite this position, however, there is a tacit understanding within the field that its role should be limited: epidemiology collects, analyses and even interprets information, but it is the public health policy-makers and agencies who should make the final risk management or policy decisions (Savitz et al., 1999). As a group of epidemiologists noted in an editorial in the *American Journal of Public Health*, "Our role is not only to collect and analyze data but also to interpret them so that they have meaning for the public, for clinicians, and for policymakers (Koplan et al., 1999)". Not all epidemiologists agree with this arrangement (e.g. Krieger, 1999), but where they disagree tends to be centred around whether – or how – epidemiologists should present their research findings to the policy world. As some argue, their work has policy relevance as well as inherent mathematical complexities and uncertainties and therefore

[2] http://www.who.int/topics/epidemiology/en/, accessed July 15, 2011.

epidemiology's terms of engagement with the policy world must be carefully considered (e.g. Weed and Mink, 2002, Samet and Lee, 2001). Consistent in much of this discourse is the assumption that epidemiologic research is neutral but its uptake is value-laden:

> In the translation of scientific evidence, outcomes of the evaluation of data should be as observer neutral as possible…By contrast, good policy outcomes cannot be observer-neutral, since many values other than scientific evidence bear on the decisions (Brownson et al., 2010: 410).

Such an interpretation suggests a lack of reflexivity within the field, as well as evidence of boundary work, through which the boundaries of epidemiology as a "science" are actively managed and maintained by its practitioners (Gieryn, 1983).[3] At the very least, however, epidemiology's debate over policy engagement demonstrates that the field as a whole is quite aware of its close links to and reliance on the public health domain. Epidemiologists may be correct to fret about the mobilisation of their knowledge and expertise – but they are naïve when they imply that they do not influence the way their knowledge gets mobilised.

As in other areas of policy-relevant scientific investigation, the enrolment of expertise can be expected to be influenced by extra-scientific factors, a phenomenon long-since observed in the regulatory domain (e.g. Jasanoff, 1990). A relevant example from a public health policy-related field was the long-standing debate over the links between smoking and lung-cancer. Collingridge and Reeve (1986) described the pattern observed in this debate as being consistent with their "over-critical model", which remains evident across much science-for-policy:

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[3] Such boundaries have in some instances been officially maintained and reinforced. For example the European Centre for Disease Prevention and Control (ECDC) is formally mandated to assess but not manage risks. The latter is left instead to the powers of those within the European Commission or EU Member States. See http://eur-lex.europa.eu/Notice.do?val=387317:cs&lang=en&list=387317:cs,&pos=1&page=1&nbl=1&pgs=10&hwords=&checktexte=checkbox&visu=#texte, accessed July 15, 2011.

> identification of some evidence by one side with a decisive interpretation; the presentation of an alternative interpretation of the same evidence by the other side; a long debate on the merits and validity of each interpretation calling on extra evidence; the development of new types of research to test the various interpretations (130-1).

It is noteworthy that the differing interpretations in this example were not only driven by disagreements between a "benevolent" medical community and a "malevolent" tobacco lobby seeking to protect their corporate interests, but also by different intellectual disciplines. In this debate, the more traditional medical communities were quite comfortable with the view that smoking led to lung cancer but a group of genetic determinists viewed tobacco intake rather more as a confounding factor masking the influence of genetic make-up on the eventual incidence of lung cancer (Collingridge and Reeve, 1986).

If different sets of experts working with epidemiological data can disagree about the interpretation of study results, this merely demonstrates that epidemiology is like any other branch of science in not being a cohesive, consensus-filled domain. Different factions may share different vested interests, different links to power-brokers in policy- or funding-worlds, and so on. Moreover, the growth of the field in past decades means that the commitments of various sub-disciplines may differ so much to be preventative not of disease but consensus. Molecular and environmental epidemiology, for example, work with different models of causation, different methodological tools, and different standards of proof. They are ideally complementary but potentially confrontational.

It is, however, not only the *uptake* of epidemiological knowledge that is a messy mélange of science and politics. The interrelationships between epidemiology and public health create the context in which epidemiologic research is conducted in addition to the one in which it is applied. The very construction of this knowledge is inherently political – and only partially because epidemiological research is so often conducted and presented with its potential usages in mind.

As discussed in Chapter 2, science and political order have been shown to co-produce one another at many different levels of analysis. Liberal Western states have tended to view science as a solution for legitimising political systems (Ezrahi, 1990) and, similarly, dominant socio-political orderings have provided solutions to the legitimization of scientific systems. This has previously been demonstrated for epidemiologic research. Classifications of race, for example, embed certain norms and values. Where these are incorporated into epidemiological surveys, they inform particular epidemiological understandings, such as about the risks of cardiovascular disease, which in turn solidify and legitimate the classifications used in the survey – even though they may gloss over more nuanced or even alternative understandings of race (Shim, 2005). Similarly, the development of the International Classification of Disease (ICD) has emphasised a particular cultural understanding of disease which is further stabilised by research using statistics extracted from ICD. Thus working with ICD:

> enforces a certain understanding of context, place, and time. It makes a certain set of discoveries, which validate its own framework, much more likely than an alternative set outside of the framework… (Star and Bowker, 1999: 82).

Belief systems and power structures shape categorisations, which in turn influence knowledge-generation in a mutually stabilising relationship.

Epidemiology, then, as public health's basic science is as a contingent form of expert knowledge, influenced not only by different disciplinary perspectives and political commitments but also broader socio-political contexts. With this in mind, the re-framing of climate change as a health risk will be examined. Notably, its re-framing mirrors the focus of the EID worldview (1.3) as well as the transition to global health governance (Chapter 2), insofar as it was viewed as a reorientation of the epidemiologic research towards a more holistic – more global – understanding of public health.

3.2.2 Escaping the proximate prison

Tony McMichael has almost certainly been the most influential figure in the CCH community. His essay, *Prisoners of the Proximate: Loosening the constraints on epidemiology in an age of change* (McMichael, 1999), offers an excellent insight into the theoretical and philosophical considerations underpinning much CCH research. McMichael suggests that a "social and environmental holism (Ibid.: 889)" characterised public health in the 1800s but the establishment of the germ theory of disease, followed by early ideas about genetics, nutrition and cancer, eventually led to narrower conceptualisations of health. By the early twentieth century, "Disease causation could … be interpreted in terms of proximate exposures and attributes (Ibid.: 889)". By the mid 20th century, despite the efforts of a few isolated epidemiologists interested in social inequalities in health, developed world epidemiologists gradually moved away from studying infectious diseases to studying complex chronic diseases. The empirical, statistical approach required to do so encouraged:

> a growing preoccupation with the role of multiple proximate risk factors… We have been busy reacting to our consumer society's procession of new, potentially hazardous exposures: mobile telephones, vitamin supplements, mad cows, photochemical smog, and endless new chemicals and drugs (889-90).

McMichael lists four key constraints hindering modern epidemiology: a preoccupation with proximate risk factors, a focus on individual-level rather than population-level influences on health, a modular view of how individuals undergo changes in risk status, and a methodological inability to address "unfamiliar global-scale environmental changes (Ibid.: 895)". These constraints restrict "the social usefulness of the research (Ibid.: 887)". In order to overcome them, McMichael points to an emerging area within epidemiology that involved fostering a more "ecologic (Ibid.: 890)" view of how social and environmental conditions influence population health.

Viewing health from a socio-ecologic systems perspective involves incorporating proximate and distal risk factors into analyses that integrate "macro-, meso- and micro-levels (Ibid.: 890)". As concerns the constraints related to large-scale, social and environmental changes, McMichael specifically cites climate change and ozone depletion. To loosen the constraints preventing analyses of these topics, he suggests two categories of research. One is empirical studies to better understand the causal links between environmental variables and health outcomes. The other is integrated mathematical modelling of future health outcomes.

There is an urgent pre-millennial tone to this essay. Infectious diseases have continued their "apparent reemergence (Ibid.: 896)" and "burgeoning human numbers and economic activity (Ibid.: 896)" are placing pressures on the global environment. Thus, "As we enter a new century, we epidemiologists must broaden our causal models (Ibid.: 895)". Only by engaging in "future-oriented interdisciplinary research (Ibid.: 895)" can epidemiologists "guide the development of proactive policies to constrain these large-scale environmental challenges (Ibid.: 895)". *Prisoners* can also be read as a subtle attack on the dominant economic order just as it was observed that the emerging diseases worldview (1.3) was "in many ways a wholesale condemnation of the consequences of modernity (King, 2002: 768)". McMichael argues that the contemporary Western consumer society has diverted the focus of modern epidemiology while also creating large-scale environmental pressures. It is a recurring theme in his work in this field, mirrored by the title of his 1993 book, *Planetary overload: global environmental change and the health of the human species* (McMichael, 1993).[4]

CCH and eco-epidemiology

In believing that the limits of epidemiology must be transcended in order to address climate change and other global-scale environmental issues, McMichael and the CCH community have pushed against the boundaries of "traditional"

[4] "I wrote it probably out of the sense that 'look this is going to be a huge issue and none of us can see it, so I will do my best to write something and try and then give it a bit of profile.'" (McMichael Interview)

epidemiology. This has paralleled a broader movement aimed at developing and pursuing the so-called eco-epidemiological paradigm (Susser and Susser, 1996). This paradigm was in turn influenced by work dating back at least as far as the earliest twentieth century focused on "disease ecology". Major Greenwood, for example, was the first professor of epidemiology at the London School of Hygiene and Tropical Medicine (LSHTM). He held the chair that Tony McMichael would later occupy and argued as early as 1919 that epidemiology had become oversimplified and reductionist (Anderson, 2004). A loose network of scientists would continue to emphasize the need for "holistic" or "systems" thinking in epidemiology throughout the 20th century, including Macfarlane Burnet, winner of the 1960 Nobel Prize for Medicine,[5] Theobald Smith, and René Dubos (Anderson, 2004).[6] Yet it was only in the late 20th century that eco-epidemiological thinking began to gain wider acceptance: "During the 1990s, amplified concern about emerging infectious diseases, along with fears of increasing antibiotic resistance and the health effects of climate change, would boost interest in disease ecology (Anderson, 2004: 61)".

According to eco-epidemiologists and CCH researchers, the "ecologic" perspective is fundamentally different from "traditional" epidemiology. It is more complex and more interested in social rather than the individual risk factors:

> **R18**: Taking into account all the environmental and social and personal risk factors makes it so much more difficult, and this is where the field of epidemiology reaches its limitations, because… traditional epidemiology is … in a way framed from a "Milton Friedman" perspective, where we look at individual level risk factors, where you, as an individual, are responsible for

[5] http://www.nobelprize.org/nobel_prizes/medicine/laureates/1960/burnet-bio.html, accessed October 31, 2011.

[6] Anderson (2004) and March and Susser (2006) provide more detailed overviews of the development of ecologic thinking in public health. They track the roots of eco-epidemiological thinking across numerous epidemiologists working in the early- to mid-20th century. These also include John Gordon, Frank Fenner, Gunnar Inghe, Thomas Francis Jr., Alexander Leighton, Manfred Pflanz, and John Cassel.

your health. However, from the ecologic perspective, you're trying to talk about societal accountability.

R20: I think, at least in Western societies, we carry a false model of what the determinants of health are. It tends to be rather individual based and we've never given very much attention to global environmental influences…

…And that is a challenge to epidemiologists because most modern epidemiologists have been brought up … studying smoking and drinking and oral contraceptive use, wearing seat belts, and safe sex and things like that - all things can be measured in the individual level. But they have not asked the bigger questions about what are the risks for whole populations.

Indeed, in its iteration in the 1990s, eco-epidemiology was specifically developed as a paradigmatic response to the more individualistic and geneticised approaches to health. In their influential paper, Susser and Susser write:

Without conscious countervailing effort, that [molecular] paradigm will very likely come to dominate epidemiology no less than did the germ theory in its time. In that event…the mainstream of our subject could be lost to creative science. A countervailing force…resides in a developed version of the Chinese boxes [eco-epidemiological] paradigm (Susser and Susser, 1996: 676).

In this century, epidemiology and public health have often withered in a medical environment that almost inevitably must give primacy to the individual care of sick persons who solicit care (Susser and Susser, 1996: 677).

Demarcating ecological approaches to epidemiology from "traditional" epidemiology requires a carefully balanced stance, as it must maintain a central role for epidemiology and a role for the study of proximate, causal relationships, while simultaneously advocating increased multi-disciplinarity and methodological innovation. There is, additionally, a normative impetus behind this movement insofar as it is concerned with being socially useful research, amenable to the needs of pro-active policy-making (to be further explored in 3.2.3). Furthermore, as discussed above, the eco-epidemiological paradigm offers resistance to reductionist, individualistic approaches to public health, which in turn have been influenced by the organisation of contemporary

Western economic life. If these philosophical and political underpinnings have informed and influenced CCH research, its emergence and stabilisation has also relied upon extra-curricular work on the part of its early researchers. To secure the success of CCH research, they needed to create a community, gain credibility within epidemiology, ensure funding, find allies within prominent public health and climate change governance institutions and, all the while, create, maintain and dismantle disciplinary boundaries.

3.3 A suitable niche: On the arrival of climate change and health

3.3.1 Epistemic communities and constructivism

As discussed in detail in 2.2, Haas (1992) described epistemic communities as a network of professionals with expertise in a specific area, sharing similar normative and causal beliefs, notions of validity and a common policy enterprise. It was argued that viewed through an STS perspective, this concept is useful for this research, as it draws attention to the ways in which science and politics become embedded. Studying epistemic communities through an STS lens means noting that "science-in-the-making" (Latour, 1987), "community-in-the-making" and "policy-in-the-making" are so intertwined that they are effectively inseparable. Perceived in this way, the epistemic community concept offers a useful analytical focus for understanding "how scientific networks and institutions form, how they come to speak with a unified voice, and how they acquire political influence (Miller and Edwards, 2001: 24)". The remainder of this Chapter asks: how was the knowledge sustaining the CCH community produced, and how did the community's normative commitments influence the production of this knowledge? How did the CCH community manage its political infiltration into governing institutions? Which "external" factors were necessary for this to happen?

3.3.2 Community service: The motivation and composition of the CCH community

One of the first peer-review publications linking climate change with health issues was an essay in the *New England Journal of Medicine* published in 1989 (Leaf, 1989).[7] "At the time", wrote the late Paul Epstein, former Director of the Center for Health and Global Environment at Harvard Medical School, "few in the health community, let alone the general public, were aware of these threats (Epstein and Ferber, 2011: 34)". Indeed, as far as climate change was concerned, its health implications were not part of the debate throughout much of the 1990s:

> **R20:** somehow in those early days, in the early 1990s right through the 1990s, it really wasn't understood that if climate change was going to continue in ways that seemed possible, it would actually weaken and erode the life support systems the human health depends on. So it always seemed obvious to me that human health was a major part of the story - but it wasn't obvious to others.

With virtually no research in this area, embarking upon it was therefore laden with risk:

> **R10:** that's how it is in science, it's safer to stay within a specific field that you know, everyone knows about

There were also opportunities. One appears to have been the ease with which the community was able to connect to each other and shape the field:

> there were only one or two names that I became aware of in the literature, whom you might have then made contact in the early 1990s, and a couple of us changed e-mails and ideas about this topic of climate change and health (**R20**).

Such interactions led to the establishment of a fairly tight-knit community. Many of the same names are cited by both insiders and outsiders as being part of a "core group" of researchers in this field:

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[7] Ironically, this publication does not come up in a search of the MeSH terms "climate change" or "global change" or "global warming" and "health" in the PubMed search engine (http://www.ncbi.nlm.nih.gov/pubmed), perhaps because the title contains "climatic" instead of "climate".

R19: And of course one of the major drivers, as you know, was Tony McMichael and is Tony McMichael.

R10: The core group, well the first person I would name... really one of the major, major players there is Tony McMichael… and then you have in USA it's Paul Epstein at…Harvard Medical School…and then you have some that focus on the more specific like Jonathan Patz has been looking at the infectious diseases and climate change in US…and in Europe because of Tony McMichael, the group around him at London School of Hygiene & Tropical Medicine, Sari Kovats and all the people around, Andy Haines, everyone there has contributed tremendously… and Pim Martens, was in Holland, a mathematical scientist in the Maastricht University…

R13: If you look up Patz, you look up McMichael, if you look up Woodward, Hales, etc ... they are the clique.

CCH research has now been around for over twenty years, but many of the initial members of the community remain both active and influential. One interviewee, who was active at the beginning but has subsequently altered course, reflected upon on the evolution of the field:

R15: It surprises me that I still see the same people in these publications … Paul Epstein, Jonathan Patz, Tony McMichael, Elizabeth Lindgren, there are a few younger people that are actually doing the research but I think not much has changed … They all know each other.

The CCH core group, or at least inner circle, is certainly broader than the handful of names mentioned in this study. For the purposes of this dissertation, however, it is useful to introduce the names of a few of the key players (**Table 4**). Most certainly know of each other and, often, they know each other personally.[8] Collectively, they have co-authored papers, organised conferences and workshops, participated in WHO task forces, written and reviewed IPCC assessments, and so on.

[8] "We have met many times before... (**R10**)".

Table 4. Selected CCH community members

Name	Affiliation	Nationality
Kristie L. Ebi	IPCC	USA
Paul Epstein (deceased)	Harvard University	USA
Howard Frumkin	University of Washington (formerly US CDC)	USA
Andrew Githeko	Kenya Medical Research Institute	Kenya
Andy Haines	LSHTM	Britain
Simon Hales	University of Otago (formerly WHO)	New Zealand
Sari Kovats	LSHTM	Britain
Elisabet Lindgren	Karolinska Institute	Sweden
Pim Martens	University of Maastricht	Netherlands
Bettina Menne	WHO (Regional Office for Europe)	Germany
Tony McMichael	Australian National University (formerly LSHTM)	Australia
Jonathan Patz	University of Wisconsin	USA
Jan Semenza	ECDC	Switzerland / Sweden / USA
Alistair Woodward	University of Auckland	New Zealand

Community commitments

Having a shared enterprise is a common characteristic of epistemic communities and it is evident within the CCH community. To begin with, the choice to work on climate change necessarily involves accepting the dominant discourse surrounding global climate change (1.4). Many interviewees from the CCH community explicitly expressed their concerns with a perceived a lack of progress in establishing global targets for greenhouse gas emission reductions (e.g. **R1, R10, R18, R19, R20**), while others have taken active stances in related issues, such as peak oil (Frumkin et al., 2007). Perhaps more profoundly, membership in the CCH community involves, for many, pursuing a research programme specifically aimed to influence policy:

> Research into the existence, future likelihood, and magnitude of health consequences of climate change represents an important input to international and national policy debates…. The evidence and anticipation of adverse health effects will indicate priorities for planned adaptive strategies, and crucially, will strengthen the case for pre-emptive policies (McMichael et al., 2006: 866).

Pre-emptive policies, from the standpoint of the CCH community, might related to climate change adaptation strategies for health, as well as policies aimed at climate change mitigation more generally. It is striking that in some cases CCH research has been specifically aimed at influencing climate change politics. As one interviewee commented concerning studies modelling the impact of climate change on infectious disease:

> **R19:** I guess my major hope for that kind of modelling was that it would be a stimulus for governments especially to take climate change more seriously and as a kind of a stimulus for mitigation action especially.

This demonstrates circularity in the relationship between climate change politics and CCH research – it has been designed to both anticipate and influence policy. Such motivations can be seen to have influenced how CCH modelling research has been undertaken:

> **R18:** I am not trying to figure out the third decimal behind the dot in terms of my estimates. I would rather try to see solutions, benefits, trying to analyse current policies and how robust they are or how flexible they are given certain [climatic] changes.

It is important to note that there are at least two other key components to the standard CCH work programme, at least as related to epidemiologic research, that were articulated by McMichael in the early 2000s. One is better understanding the causal links between various climate variables and health outcomes based on recent historical examples, and the other is to enhance the monitoring of current examples of health outcomes that have already been influenced by climate change (McMichael, 2001a). Yet the intense data requirements and statistical complexities of isolating the influence of climate *vis à vis* other potential risk factors via "traditional" epidemiologic methods means that scenario-based models have played a substantial role within the field (3.3.4, Chapter 4). Thus it is both normative commitment and practical necessity that has driven the development of future-orientated models for much CCH research.

Consistent with the community's commitments, its practitioners have sought to influence policy through their research choices and through actively translating their research by engaging the media and political circles both nationally and internationally. They have simultaneously justified the case for CCH research through editorials and commentaries in scientific publications. There is both idealism and opportunism in this field-expansion type of work:

> **R10:** ... I think it's important if you are a scientist and you have important things to tell, you should tell them… especially when we are dealing with those kind of… extremely important issues for the future of planet, you should actually take your time and translate what the scientific knowledge into normal language so that normal people and the policy makers can understand what you are talking about…

> **R20:** Well I and some of my closest colleagues have taken a lot of opportunities but in setting very different settings, whether it is writing pieces for broadsheet newspapers, doing media interviews, speaking with politicians, speaking with lots of community based organizations that want to hear about this… you take opportunities to write books, write other pieces that might have a bit of influence, and speak with whatever politicians and policy makers you can.

These activities have involved a keen situational awareness. As concerns both the popular and scientific media, one angle has been to specifically deploy the "EID worldview" to draw attention to climate change as a risk factor for health:

> **R20:** ...you know we all … I suppose, carry dark atavistic fears about infectious disease - that it's got this sort of miasmatic property that is sweeping over populations, which is why there's been such a panic about H1N1. So that's sort of embedded in the human psyche, and that is a good one to appeal to.

This appeal appears to have worked, even occasionally in the mainstream media. McMichael, for example, is quoted talking about EIDs in a *Guardian* article entitled "New threats to health predicted" from 2004 (Meikle, 2004) and in 2001, under an article entitled "Melt down" (Brown, 2001), McMichael's book *Human Frontiers, Environments and Disease* (McMichael, 2001b) is discussed at some length.[9] Others in the CCH community have been occasionally represented in the media; as just a few additional examples, Paul Epstein is a key source in a *New York Times* article entitled "Is climate change aiding spread of disease? (Smith, 2002)", and Jonathan Patz and Paul Epstein are both cited in a *Washington Post* article entitled "Infectious disease may rise as the world gets warmer (Brown, 1996)".

Applying more sophisticated language and subtler framings, the CCH community has devoted the bulk of its energy to awareness-raising across the epidemiologic, public health and medical communities. Prominent journals are full of comments, editorials, perspectives and letters authored and edited by those within the CCH community. This has included awareness-raising essays in popular science journals such as *Scientific American* (Epstein, 2000), calls for additional funding (Ebi et al., 2009), and countless editorials, commentaries and reviews aimed at informing the public health and medical community about the state-of-the-art in CCH issues. These have been published in internationally and nationally recognised journals including *Science* (Epstein, 1999), *Lancet* (Haines, 1998, Hales, 2003, Epstein, 1998, McMichael, 2000) the *British Medical Journal* (Haines, 1995, Haines and McMichael, 1997, McMichael et al., 2008, Menne and Bertollini, 2005), *JAMA* (Haines and Patz, 2004, Lupo and Hagan, 2008, Wiley and Gostin, 2009), the *American Journal of Preventative Medicine* (Bloomberg and Agarwala, 2008, Ebi and Semenza, 2008, Frumkin et al., 2008), the *Bulletin of the World Health Organization* (Martens et al., 1997, Corvalan and

[9] For example, this article states: "On the whole, the picture is one of increasing and more uncontrollable disasters, almost unmitigated bad news for the poorest and already disadvantaged developing world, and hardly much better for anyone else. In his book … *Human Frontiers, Environments and Disease*, Tony McMichael, professor of epidemiology, at London School of Hygiene and Tropical Medicine paints a dire picture of what global warming is letting us in for".

Patz, 2004), the *International Journal of Public Health* (Kovats, 2010), the *Canadian Medical Association Journal* (Haines et al., 2000, Kovats and Haines, 2005), *Läkartidningen* (the Swedish Journal of Medicine) (Lindgren, 1995, Lindgren, 2000, Lindgren et al., 2008), the *Australian and New Zealand Journal of Medicine* (Woodward, 1995) the *Medical Journal of Australia* (McMichael and Butler, 2009) and the *Nederlands Tidschrift voor Geneeskunde* (Dutch Journal of Medicine)(Martens, 2009).

There is, as Brisbois and Harris Ali (2010) have observed, generally a high level of homogeneity in the message emerging from this literature. It often begins with a discussion on the threats posed by climate change and it also tends to call for increased – and, often, interdisciplinary – research to further explore the links between climate change and health. An excerpt from the abstract of a review article published in *JAMA* in 1996 is fairly illustrative:

> Analyzing the role of climate in the emergence of human infectious diseases will require interdisciplinary cooperation among physicians, climatologists, biologists, and social scientists. Increased disease surveillance, integrated modeling, and use of geographically based data systems will afford more anticipatory measures by the medical community. Understanding the linkages between climatological and ecological change as determinants of disease emergence and redistribution will ultimately help optimize preventive strategies (Patz et al., 1996: 217).

If the CCH community's active work to communicate and establish its *modus operandi* within public health demonstrates its shared scientific and policy commitment, then the content of this work demonstrates its influences and priorities. Both have played a role in carving out a niche for CCH within the epidemiology and public health world; both demonstrate a persistent effort to draw boundaries around a new a discipline while also establishing ownership over it; both were essential for legitimising a topic that faced resistance its early days.

3.3.3 Building bridges

As discussed in 3.2.2, eco-epidemiological approaches represent a departure point from "modern" epidemiology. One of the key early challenges for CCH research was establishing its scientific legitimacy. Two sources of doubt in particular needed to be addressed among funders and scientific peers. One related to the seriousness of climate change itself as an issue; the other to its relevance and amenability as a health topic:

> **R20:** It was...right through the 1990s, it was pretty difficult to get this sort of work funded because the topic seemed unfamiliar and complex, there were a lot of people that were still saying, understandably because this was the 1990s, well we don't know if (climate change) is real and we don't know if it is serious and of course there were many people out there saying, well, what type of research is this?

To some extent, the former became less of an issue as climate change became more and more broadly accepted into the mainstream of scientific and political discourse. Nature itself was occasionally helpful: many interviewees cited climatic events as key strategic allies. Heat waves in Chicago in the 1990s and in Europe in 2003, an ice storm in Canada, a storm devastating trees in Sweden, flooding in the UK and Central Europe – all were, to varying degrees, linked to popular and political discourses surrounding climate change. Each, according to respondents, helped raise the profile of climate change generally, thereby contributing to more favourable research and policy climates.[10]

Overcoming the doubts about the relevance of CCH proved to be a more substantial barrier to the field:

> **R20:** So it wasn't easy to be trying to work on those kinds of issues because it was kind of going beyond what was accepted as part of traditional

[10] This was a fairly consistent message across interviewees, even if the academic literature studying media coverage of climate change tends to suggest that its overall coverage has a mixed influence on the direction of popular belief about climate science. See, for example, (Boykoff & Boykoff, 2004, Carvalho, 2007).

epidemiology I suppose. So yes, convincing funders and scientific peers and so on that it was a valid approach was not easy.

From the onset, there were, broadly, two main focuses for research contributing to the CCH evidence base. One was establishing the links between climate – or weather – and health outcomes, to better understand the links. The other was scenario-based modelling:

> **R8:** I mean there are different types of evidence base - it is quite complicated. You've got...we know climate factors affect health now, and that sort of evidence-base has always been there, because we know about floods, a bit about heat waves, and malaria... Everyone knows how climate affects malaria - pretty much… talking about projection studies, that is a different type of evidence ...about sort of policy relevant evidence that people look and say, 'Oh no its is going up in my county'.

One of the central purposes of the CCH research examining the links between climate and health was to develop parameterisations which could then feed in to models:

> **R10:** so at that was at that time in the 1990's when we did these studies it was a way of showing that there are linkages there was a showing that out in nature things are happening … and after that you can find ways of doing modelling to look at the future trends

One of the many balancing acts that CCH has embarked upon was to deploy an interpretative flexibility between "weather" and "climate". It is noteworthy that much of the evidence-base on linkages between climate and health has in fact been based upon the former:

> **R10:** … long before climate change was … even a thought … people were looking at which temperatures are essential for certain vectors, insects … what parameters are important for the malaria mosquitoes to survive and so on…
>
> **R18:** It is true though that ... we do tend to look at individual weather events instead of climate change because we use a reductionist approach in science, which makes it much easier for us to come to this cause-effect conclusion ...

Such a reductionist approach, of course, is not how one would describe the modelling activities, in which incorporation of weather parameters into projections of climate change effectively converts "weather" into "climate". Yet this "linkage" research provided necessary inputs for modelling efforts and, furthermore, was in practice reasonably compatible with "traditional" epidemiology:

> **R20:** The sort of work that has thrived most easily, which you might think of as sort of low-hanging fruit work, has been for many of the epidemiologists who in the 1980s and 1990s were working on air pollution episodes and the acute effects on health. They found it easy to move ... straight into looking at heat wave episodes and the acute effects on mortality on health hospitalization - the same methods… dozens of environmental epidemiologists with air pollution backgrounds have moved into that...

Other examples include time series data in relation to food poisoning by salmonella and campylobacter; temperature fluctuation and diarrheal episodes; and the influence of cyclical climatic events like El Niño, for example, on malaria outbreaks. Again, this sort of work "was undertaken reasonably comfortably and it could be done within an existing sort of epidemiological methods framework (**R20**)".

Collectively, such research helped to gain CCH a foothold whilst it grappled with more profound issues related to modelling the future. The "traditional" epidemiologic community, accustomed as it was to proximate risk factor analysis, was perhaps neither willing nor able to come to grips with future-orientated approaches:

> **R20:** ... if you want to study future risks: no testable hypothesis there, you won't be around in 2050... So there was all sorts of debate and resistance to it [CCH research], I think, in the late 90s.

> **R6:** The health sector by and large has not liked models, has been pretty much against modelling and when you talk about modelling almost immediately you get into a discussion and this happens to me all the time on. ... It's essentially the difference between weather and climate. 'Gee, if you can't tell me if we have

> malaria next week, how can you tell we'll have malaria in 2100?' Different issues, and there's not a clear understanding that these are very different issues.

This is exactly the sort of problem that one might expect given the paradigmatic struggles within epidemiology (3.2.2). For its modelling work, the CCH community necessarily borrowed methodological approaches and data, such as IPCC-endorsed climate models, from the climate change community. Such activities, clearly multi-disciplinary, led to the uncomfortable but necessary situation in which they needed to translate their work to both health and environment communities:

> **R15:** It was the very funny thing that people in the environment field, climate change business they knew the value of models. They also were used to dealing with models with the complexity, uncertainties, but they had hardly any knowledge of health issues of course. On the other hand the health community knew a lot about health but they were not that much interested in long term perspectives and views on that and models as such. They didn't know how to put a value on it.

Perhaps not only familiarity with different methodological approaches, but also different standards of proof across sectors contributed to the initial reluctance of the health sector to accept such models:

> **R8:** we [epidemiologists] are drilled and drilled about evidence-base and not making... you know, unless, you've got a very good basis for doing your modelling, for not doing it. And the other sectors do not apply those standards – not to the same effect. For some of the areas are very weak, but yet a lot of the modelling is done by economists, or just modellers, who don't have the same rules - they are not playing the same game as us.

There was in fact a fear amongst the CCH community that it was being left behind other climate change impact sectors, in part because public health and epidemiology are more rigorous. As a result, some perceived that the health sector was missing an opportunity in climate change policy discourses to obtain resources and to emerge as a priority area.[11] The history of health coverage within IPCC Assessment Reports lends some credence to this suspicion, particularly as in the 1990s health was only a very marginal part of the IPCC assessment process (to be further discussed in 3.3.4 and Chapter 5).

Another factor limiting the growth of the CCH field related to funding and publishing. Once again, it was a challenge to get CCH work to be seen as acceptable:

> **R6:** The hard part is nobody has paid the money to build the model. And then trying to get funders to ... understand the complexity of these systems.

It is worth noting that not all CCH research is expensive in comparison to other branches of epidemiologic or public health research. CCH is not amenable to long-term cohort or case-control studies, for example, which tend to be the costliest types of epidemiologic research. Nonetheless the acquisition of all of the data needed to inform models, the international collaborations that are often part of the research, the assembly and training of interdisciplinary teams, and the time it takes to develop and run models are expensive – external funding is needed. CCH initially had a hard time getting funded, and one reason is that it fit uncomfortably within the mandates of both health and environmental funding bodies:

> **R6:** If you look at almost all agencies, they still fund within stove pipes ...and it's a constant challenge.
>
> **R15:** I also mentioned earlier to you that we are trying to get in a kind of national funding programme at [the] National Academy of Sciences on Climate Change and Health. But normally a Ministry of the Environment or a Ministry of Health they should be the kind of persons to get it up and started but you are being referred to one or the other because ... then they say, "Well it's about

[11] As one interviewee noted at preparatory meeting to COP15, also held in Copenhagen: "The emphasis was on us to really start getting some work done and work out there because otherwise you know we would be seen as not doing what we should be doing, and it sounded a bit like the drive to get research done was mostly from the fear of being left behind (**R3**)".

> health, so go to the Ministry of Health," and they address us back to the other... So it is in between ...

Perhaps because of its greater familiarity and acceptance of CCH methodologies, the environment sector appears to have been a more fruitful home for the initial funding of CCH research than was the health sector.[12] A similar situation happened with the publication of early CCH research, particularly from scenario modelling:

> **R15:** [we] mainly published in the early stages in environmental journals like *Climatic Change* … *Global Environmental Change*, these kinds of journals. The more medical journals, they were… at first quite sceptical of these modelling approaches.

Once again the CCH community was also pro-active, launching new academic journals, such as *EcoHealth* and *EcoSystem Health*, which were specifically designed to "give more attention and to provide a platform for publishing work on the interface between global environmental changes and population health issues (**R15**)". With a platform to publish studies, early results from "reductionist" studies, a growing community and growing general awareness about climate change, CCH began to carve out a comfortable, if somewhat financially constrained, niche which it could use as a platform for further expansion. This enabled – and was further enabled by – its infiltration of the public health and climate change worlds.

3.3.4 Infiltration: CCH at WHO and IPCC

CCH research is hybrid in the Latourian (1993) sense: it simultaneously embeds both "scientific" and "political" elements. Given its content and its policy-focus, the emergence of CCH research eventually also led to the formation of what Brown and Michael have called "institutional hybrids" (Brown et al., 2006,

[12]As an interviewee mentioned: "People don't fund global model development. The health funders don't fund it because they don't see the point, you know, because it does not have a direct benefit to health interventions. Whereas the environment, I've got a lot of money through environmental…(**R8**)"

Brown and Michael, 2004): it created a need "to alter the boundaries between existing institutional arrangements (Brown and Michael, 2004: 208)". The focus of this and the next section will be to explore how these arrangements were altered, paying attention to the role of the CCH community as well as those of the broader global governance structures surrounding climate change. It will be shown that both aligned in such a way that CCH research became entrenched within the global governance landscape.

CCH and the UN

With an eye to producing policy-relevant knowledge, there was no better home for CCH work than WHO. A foothold there would help raise the awareness of CCH as a "political" issue while also contributing to its entrenchment in the research world. However, mirroring the challenges CCH faced in gaining recognition across the health sector, WHO did not initially embrace the CCH community with open arms. Reflecting back on the acceptance of the CCH topic by WHO:

> **R6:** … and so it's been a long road to try and get people to understand, particularly given all the other issues WHO deals with, that they should also take climate change on board.

In the early 1990s, it was by no means obvious that the WHO would one day embrace climate change forcefully. By the mid-1990s, WHO had decided to embark upon CCH but it was still a rather marginal topic within the agency:

> **Jonathan:** And how was climate changed perceived then as an issue within WHO?
>
> **R2:** An exotic subject. Not very robust science, not very clear on what could be done on the subject… you know quite confusing on what the word adaptation is… What is mitigation?

Two factors relating to WHO's position in the world nudged it towards further engagement with climate change, playing to the advantage of the CCH community. One factor is that WHO, as discussed in 2.3, has been operating in a fractured global health governance landscape characterised by increased competition among agencies, poor coordination, and overlapping organisational mandates (e.g. Walt, 2001). One interviewee, part of the CCH core group, had recently returned from spending a year at WHO in Geneva when interviewed:

> **R19:** I mean WHO wants to maintain its kind of status as a leader in global health and that's understandable… And there were various - as you know, other sort of major organisations kind of have taken over some of that field and particularly the World Bank and the Gates Foundation.
>
> So I think there is a feeling that WHO needs to show that it's a leader, at least in some areas of global health. And I think this is one of the ones that they've chosen, that they … can be the world expert on it.
>
> **Jonathan:** You mean climate change in general?
>
> **R19:** Yes. I think so. And I think that partly comes from within WHO and that partly comes from the sort of international process that I mentioned.

The "international process" is the UN process. WHO's role as a UN organisation means that WHO is drawn into UN political agendas and processes, including those surrounding climate change. These in turn can influence WHO prioritisation, in some cases formally through World Health Assembly (WHA) Resolutions which mandate the WHO work programme.[13]

In 1990, for example, WHO was asked to contribute some information on health to the IPCC First Assessment Report (ARI), which it did so via a report (WHO, 1990). Although the findings from this report (which was not produced in collaboration with the CCH community) did not ultimately end up being covered

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[13] The World Health Assembly is "the decision-making body of WHO. It is attended by delegations from all WHO Member States and focuses on a specific health agenda prepared by the Executive Board. The main functions of the World Health Assembly are to determine the policies of the Organization, appoint the Director-General, supervise financial policies, and review and approve the proposed programme budget. The Health Assembly is held annually in Geneva, Switzerland". From: http://www.who.int/mediacentre/events/governance/wha/en/index.html, accessed October 31, 2011.

in ARI, it marked the beginning of WHO engagement with CCH. Two years later, WHO was asked to contribute to the 1992 Rio Earth Summit, marking the beginning of more serious WHO involvement in CCH research. The Rio Summit would lead to the adoption of three conventions, including the UN Framework Convention on Climate Change (UNFCCC)[14] and would have two major impacts on WHO and its attention to climate change. It provided an occasion for WHO to consider the topic in detail, and its eventual wording would create yet further impetus for WHO involvement:

> **R2:** The whole climate change debate, I mean as a subject itself, was put at the first time in the internal WHO discussion before the Rio conference, actually….
>
> …So then it was taken up in Rio and of course in the convention on climate change (UNFCCC) … as you can see Article 2 is actually referring to human health and wellbeing.[15]

The UNFCCC, focused on addressing anthropogenic climate change, mandates signatories to develop policies and conduct impact assessments "with a view to minimizing adverse effects on the economy, *on public health* and on the quality of the environment (emphasis added)".[16] This wording, referring to public health as it does, may have contributed to the interest in health in other UN-related initiatives focused on climate change. As will be examined in greater detail in 5.3, by the time IPCC started the process for its Second Assessment

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[14] The other two conventions were the Convention on Biological Diversity and the Convention to Combat Desertification. These conventions were adopted by 192 countries. http://unfccc.int/essential_background/feeling_the_heat/items/2913.php, accessed August 5, 2011.

[15] In fact, Article 2 does not explicitly mention public health, but Article 4 does. Article 2, which outlines the objective of the Convention, states: "The ultimate objective of this Convention and any related legal instruments that the Conference of the Parties may adopt is to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system. Such a level should be achieved within a time frame sufficient to allow ecosystems to adapt naturally to climate change, to ensure that food production is not threatened and to enable economic development to proceed in a sustainable manner". See: http://unfccc.int/essential_background/convention/background/items/2853.php,, accessed Aug. 5, 2011.

[16] http://unfccc.int/essential_background/convention/background/items/2853.php, Commitment 1f), accessed August 5, 2011.

Review (SAR), which was eventually released in 1995, health was allocated a more prominent position:

> **R20:** Anyway, the second time around they ... wanted a full chapter on human health, which was a sign that there was an emerging awareness. It might have reflected the fact that if you go back and look at the original wording of the UN framework convention on climate change... because it talks about the need for coordinated international action to avoid dangers interference with the climate system and it offers as a reason for that, the need to protect economic development, human health and the environment.

Thus the global political discourse surrounding climate change, something which the CCH community had extensively and explicitly incorporated into its research programme, played a role in anchoring CCH research. Yet it was the activities of the CCH community that created this opportunity and were by then in the right position to take advantage of it. They had by then achieved enough of a profile to be invited to produce a report for WHO and to present their findings to IPCC.[17] After this presentation, Paul Epstein notes, "we managed to persuade them (IPCC) that our findings should be included (Epstein and Ferber, 2011: 66)" in the upcoming second assessment report. Indeed, a condensed version of these findings would form the health chapter for IPCC SAR, and an expanded version would later be published as a book by WHO. For the IPCC report, Tony McMichael was the coordinating lead author of the health chapter; among the principal lead authors were Paul Epstein, Jonathan Patz and Andy Haines, and among the contributing lead authors were Alistair Woodward, Sari Kovats and Pim Martens (see 5.3, Table 6). Having health issues clearly documented in a high-profile IPCC report would be something of a landmark for the CCH community, gaining them further influence within the global health arena:

> **R20:** I think once we got into the IPCC process and a group of us wrote that chapter, and we then discussed with WHO the possibility of expanding and

[17] Paul Epstein suggests that his invitation came from the WHO after a representative had read a series that the CCH had successfully pitched and published with *Lancet* in 1993 (Epstein and Ferber, 2010: 65).

> bringing it out as a book, which we did in 1996, on climate change and human health - we started to establish a bit of visibility.

Around the same time that WHO published this book (McMichael et al., 1996), WHO, the World Meteorological Organisation (WMO), and the United Nations Environment Programme (UNEP) began to collaborate. This collaboration would eventually be endorsed by a UN Inter-Agency Committee on the Climate Agenda (IACCAA) for which the WMO acted as secretariat during the late 1990s[18]. These activities eventually lead to the first WHA Resolution on climate change in 1998, WHA51.29 (Kovats et al., 2000).[19] Informing the WHA discussion on WHA51.29 was the work conducted by the CCH community, which itself drew upon their own earlier work for IPCC.[20] WHO also began coordinating CCH work more aggressively, more formally involving personnel from a range of departments and other agencies.[21]

The close working relationships between WHO and the CCH community continued as well, leading to a virtuous circle in which the CCH community would produce research and reports, in many instances funded by WHO. This would then reinforce the legitimacy of CCH as well as consolidate the leadership of the CCH community, but it would also help WHO to consolidate its leadership.

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[18] "WMO provided secretariat functions for IACCA. The Secretary General of WMO convened three meetings of IACCA, the first was held in April 1997, the second in April 1998 and the third in March 1999. Notwithstanding the endorsement of the sponsoring agencies, IACCA has not met since 1999". See page 29: www.wmo.int/pages/prog/wcp/cca/documents/climate_leadership.pdf, *accessed Aug. 5, 2011.*

[19] Annex 1 of this publication describes this series of events: "In 1996, WHO, the World Meteorological Organization and the United Nations Environment Programme tentatively established a collaborative network on climate and human health, which was endorsed in 1997 by the Inter-Agency Committee on the Climate Agenda, a joint programme of international agencies concerned with climate issues. In May 1998, the World Health Assembly approved these initiatives and requested the Director-General to formalize the agreements and begin collaborative actions in support of Member States (resolution WHA 51.29)".

[20] **R2**: "Then, for that event for the discussion at the World Health Assembly a first report on climate change and health was actually developed which was authored by Anthony McMichael as a first author".

[21] **R2**: "… we were altogether something like 20 of us involved into this cross cutting, you know coordinating between each other the input. So that was a new dimension too because it was more and more recognised … as a cross sectoral importance…"

The CCH community would also continue to lead the health chapters of subsequent IPCC assessments (5.3). IPCC reports, internationally renowned, carry a significant weight in policy circles and are often cited in official documents. For example, a Staff Working Document on Climate Change and Health from DG SANCO of the European Commission refers to the health chapter of AR4. Similarly, the WHA resolution on climate change and health from 2008 (WHA61.19) specifically references the human health chapter from IPCC AR4 in urging its Member States to "take decisive action to address health impacts from climate change".[22] This was a major event for the CCH community:

> **R6:** It means that WHO will now take climate change forward as an issue to its member states. It will take climate change forward as an issue to the United Nations and will make sure that all the sister UN agencies understand that climate change is a risk to health, it is something that it has to be addressed. And so it raises a priority of the issue enormously within the health sector to say that WHO has recognized, "This is indeed something that people should be paying attention to"... and then funding streams become available: there's expert meetings, there's expert reports, research gets done and so it really was an enormous shift in the field of where we're going to be going and the amount of visibility that climate change and health is going to have.

> **R2:** I mean the World Health Assembly Resolutions are the governing documents of the World Health Organization… a resolution gives both the Member State itself action points on what they could be considering to do … [and] also gives the WHO a mandate to look more into that, into the whole climate change and health issue and to develop much more technical support to the Member States.

Thus the WHA resolution created the impetus for additional CCH research at national and international level and gave further prestige to the topic. By the time of the Madrid Consultation in 2008, CCH had emerged as a priority area for public health, embraced by the IPCC and WHA and leading to a reinvigorated WHO work programme. This increased visibility for CCH within the global health and UN landscape can, to a large extent, be considered to be the consequence of mutually beneficial relationships between WHO, IPCC and the

[22] WHA 61.19. http://www.who.int/phe/news/wha/en/index.html, accessed August 5, 2011.

CCH community. These relationships contributed to the overall growth of the field and to a re-ordering across the health and environment sectors.

3.3.5 The stabilisation of CCH

Climate change and health became a topic for study through the efforts of a community of epidemiologists who re-reconsidered the risk of climate change partially through the "EID worldview". In the early 1990s, there were only a handful of people working on this, but the field gradually grew in size and in profile through the gradual release of publications, official reports and, notably, chapters addressing health in the IPCC second, third and fourth assessment reviews published in 1995, 2001 and 2007, respectively. By 2008, the CCH community consisted not only of the initial group of epidemiologists interested in more ecological approaches to the study of population health, but also officials from national and international public health and environment agencies. Much more attention and visibility surrounded the field, mirroring its growth in the scientific literature (**Figure 1**).

Perhaps ironically, it was the environment sector that was initially more responsive to the CCH community's work, likely because of its familiarity with climate-based modelling approaches but perhaps also because drawing attention to the health aspects of climate change presented a further means of stabilising "dominant" climate change science. Regardless, the emergence of CCH bridged two traditionally separate sectors and re-ordered each of them in the process. As this took place, CCH gained credibility and, relatedly, access to funding, policy actors and processes, IPCC reviews, and so on.

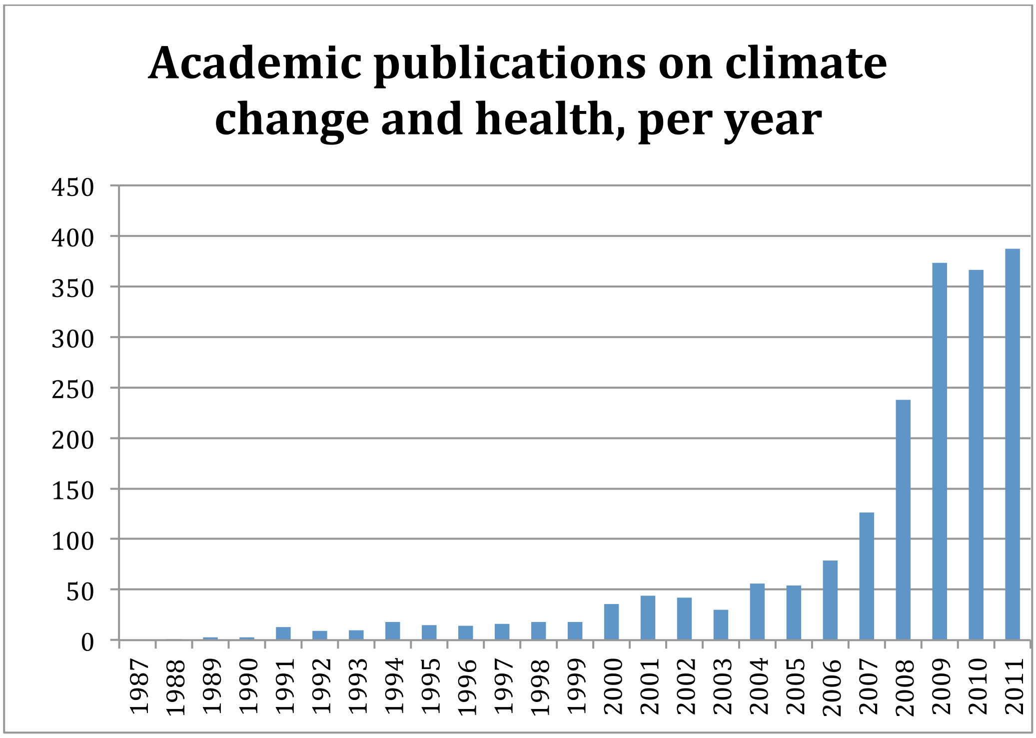

Figure 1. Academic publications per year with the search terms "climate change" OR "global warming" AND "health"[23]

There is certainly plenty of evidence to suggest that the health and environmental sectors have accommodated CCH research. The IPCC is one primary example; a WHO collaboration with WMO indicates a similar trend. By the early 2000s, WHO had re-organised so as to more explicitly address CCH and related issues. At the time, this was interpreted by McMichael as symbolic of a changing awareness in the transition to eco-epidemiology:

> The realization is gradually dawning on modern societies that the sustainability of population health must be a central consideration in this sustainability

[23] Data for Figure 1 was collected by searching "climate change" OR "global warming" AND "health" in the PubMed search engine (http://www.ncbi.nlm.nih.gov/pubmed). This includes reviews, editorials, commentaries in addition to research papers. It has been reported elsewhere that the CCH literature, as compared to that for more "traditional" epidemiological topics, like tobacco or obesity, contains a high proportion of citations that cannot be classified as original research (Hosking & Campbell-Lendrum, 2012). Although an imperfect, and likely delayed measure of growth in the field (owing to publication lag-times), it is a suitable representation of the trend in the field. It should nonetheless be stressed that the number of publications addressing climate change and health is likely much larger than what these search terms retrieved. In addition, extending the search to other databases, including those more focused on the biological sciences, would have likely greatly increased the total number of publications.

transition discourse. For that reason, the public, policymakers, and other scientists show an increasing interest in hearing from epidemiologists about these matters. Reflecting this changing agenda, the World Health Organization now has a major section titled Healthy Environments and Sustainable Development. We are edging toward a view of population health as an ecological entity… (McMichael, 2001a: 1172-3)

Other government agencies have also responded to CCH. At the national level, the US, for example, now has an Interagency Working Group on Climate Change on Health, represented by several relevant US agencies, including the Centres for Disease Control (CDC), National Institutes for Health (NIH), Environmental Protection Agency (EPA) and U.S. Department of Agriculture (USDA).[24] In Canada, Health Canada established a unit dedicated to CCH[25], while in the UK its Department of Health has held expert working groups on climate change and health, releasing large reports in 2001 and 2008 (and with key contributions from the CCH community).[26] At the EU level, as earlier discussed, DG SANCO developed a Staff Working Document on the topic, mandating work to be conducted across a variety of EU Agencies. A few staff dedicated to the topic at the European Centre for Disease Prevention and Control (ECDC), for example, as does the European Environment Agency (EEA). As concerns the latter, this has not always been the case:

R5: I think that in the beginning, even though it's kind of a common sense that one of the impacts [of climate change] would be on human health, I think it was more isolated … a, let's say, separate trend of activities related to human health… This perception is changing …

Consequently, EEA has included health as one of the climate change impacts to be incorporated into one of its key reports; the health section was developed in collaboration with WHO and the European Commission's Joint Research Centre in 2008 (EEA et al., 2008) and with WHO and ECDC in 2012 (EEA, 2012).

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[24] http://www.niehs.nih.gov/about/od/programs/climatechange/index.cfm, accessed November 1, 2012
[25] http://www.hc-sc.gc.ca/ewh-semt/climat/index-eng.php, accessed November 1, 2012
[26] http://www.dh.gov.uk/en/Publicationsandstatistics/Publications/PublicationsPolicyAndGuidance/DH_4007935, accessed November 1, 2012

Funding has, meanwhile, also opened up at the European level. In perhaps the largest project, the European Commission's DG Research funded a five year, €11.5 million project entitled *Emerging Diseases in a Changing European Environment* (EDEN) which focused on environmental and ecological change and its impact on disease pathogens. Many sub-projects within EDEN deployed predictive modelling approaches and a core objective was for the research to be policy-relevant.[27] The EDEN project was then succeeded by *EDENext*, a four-year projected funded by DG Research and conducted by a similar constellation of researchers.[28]

Such developments have contributed to an increased visibility and legitimacy of the field, as well as growth in the number of projects and researchers involved in the topic, supported by policy documents, resolutions and money. It is nearly easy to forget that CCH was initially a marginal topic within public health and epidemiology. It was a topic for which the CCH pioneers took a risk:

> **R6:**… if... you jump out into insecurity with regard to funding, especially if it's a question of trans-disciplinary, multi-disciplinary approaches… a lot of people don't dare do that and didn't in the 1990's when this field was building up

Today, with heightened awareness, the field has even become a bit trendy:

> **R20:** So this decade we have seen a shift. ... and ...to an extent it is even becoming a sort of fashionable topic now … I think once an area like this gets bit of a momentum and starts to attract a bit of research funding, others start to take notice and suddenly find that the work that they are doing could be

27 "EDEN (Emerging Diseases in a changing European eNvironment) is an Integrated Project of the European Commission that aims to identify and catalogue those European ecosystems and environmental conditions which can influence the spatial and temporal distribution and dynamics of human pathogenic agents. The project develops and co-coordinates a set of generic methods, tools and skills such as predictive models, early warning and monitoring tools which can be used by decision makers for risk assessment, decision support for intervention and public health policies". http://www.eden-fp6project.net/, accessed August 10, 2011.

28 http://www.edenext.eu/, accessed February 22, 2012. As the website for EDENnext notes: "Due to environmental and socio-economic changes, vector-borne diseases … are becoming an increasing challenge for human and veterinary public health not only in Europe, but across the globe".

> adjusted or adapted to explore some climate change and health relationships. So it has now become a bit of an attracter.

> **R6**: [there] has been kind of an explosion within this climate change field since 2001. Especially during the latest years because there has been much more funding coming in...

Even a scientist who has thoroughly opposed CCH research agrees with this assessment:

> **R17:** But my own opinion is that, it's the phenomenon one sees in science all the time, here is a hook people are grabbing hold of, which legitimises their work, is an opportunity to create new funding opportunities and to apply these funding opportunities, you know… I think, in the climate change and health debate, in the climate change debate altogether, it's a huge great area that people can make a living out of. It's not bad, it's just natural, it's human nature…

Thus climate change has been a convenient truth for the CCH community: connecting to the broader global discourses surrounding climate change facilitated its growing role within WHO and IPCC. Operating out of an initially marginal position within epidemiology, the CCH community carved out a niche for themselves:

> Now, as before, the leading instigators of…ecological approaches argue that their views are marginal in biomedical research… But theoretical speculation of this sort, once little more than a mark of intellectual distinction, a flashy bit of plumage, would eventually become a major selection advantage in the rapidly changing, and perplexing, natural and conceptual environments of the late twentieth century (Anderson, 2004: 61).

Once weakened by their move away from the mainstream, they ultimately benefited from it. The subject, once "exotic", has become somewhat mainstream: "It's like when you throw a stone into water, it causes rings…(**R6**)".

Throwing a rock into water, however, may also cause a splash and make a bit of noise, and the CCH community has had to endure some very rough waters indeed, notably from a group of ecologists who took exception with epidemiology's ecologic turn, as the following Chapter will explore.

Chapter 4. A contested niche: ecology's resistance to eco-epidemiology

"Our future lies not in the stars, but in our models"[1]

4.1 Introduction

Although the emergence of CCH served to stabilise and even somewhat re-order the public health landscape, it is important to stress that there was nothing inevitable about its trajectory. As Chapter 3 observed, CCH research has been influenced by the commitments shared by the CCH community and supported by the "anchoring" of this research within the broader climate change domain, which created an organisational impetus for WHO and later IPCC to address CCH.

This Chapter will delve into a longstanding dispute between the CCH community and a group of ecologists who have staunchly opposed the relevance of the links between climate change and infectious (particularly vector-borne) disease. An exploration of this controversy not only reveals the many contingencies inherent to CCH research, but it also raises questions about how, given that CCH research has been so intensely contested, official organisations like the IPCC and WHO have accounted for this controversy. The latter will be the focus of Chapter 5. In this Chapter, after introducing the general terms of the climate-vector-borne disease (VBD) debate (4.2), specific aspects of the controversy will be analysed in greater detail. A discussion on CCH research surrounding malaria demonstrates how different methodological choices and conclusions have aligned with professional commitments, and how CCH models incorporate a wide range of assumptions, including normative ones, about future socio-economic development (4.3). In 4.4, CCH research on "highland malaria" demonstrates how possibility for endless technical debate exists when one research community (i.e. climate impact researchers) is dependent upon

[1] Quotation by the economist William D. Nordhaus. The quote is taken from Nordhaus (1994: 6), cited in Timmerman (1996).

another (i.e. climate modellers) for their underlying climate data. In 4.5, debates surrounding climate-dengue linkages again demonstrate how different commitments and visions of the future are embedded in models, in this case even where the same methodologies are used. In 4.6, research examining the causes behind the increase in TBE incidence in Europe demonstrates how even where opponents might seem to agree with each other, their entrenched positions and concerns over how findings could be interpreted by "downstream" users can continue to stir controversy. Section 4.7 summarises the key insights arising from this Chapter.

4.2 Doctors without borders: introducing the climate-disease controversy

It is ironic that the CCH community pursued an ecological approach to epidemiology but met their greatest resistance from a community of ecologists. This resistance led to an intense and long-standing controversy over the merits and validity of CCH research, most specifically where it has focused on the links between climate change and EIDs. A special issue of the journal *Ecology* reflects a perspective on CCH research shared by many of its opponents. Suggesting that "early reviews about climate change exaggerated claims that infectious diseases will increase in the future (Lafferty, 2009: 932)" and observing that a "common message is that an ecological approach is increasingly relevant to the challenging topic of infectious disease (Ibid.: 932)", it was pointed out that if ecological perspectives on health are what is required, then ecologists could "contribute substantially (Ibid.: 933)". Although this is an eminently reasonable position, ecologists' engagement with CCH research tended to lead to controversy instead of collaboration.

The controversy surrounding CCH research has, in many instances, been highly polarised. It emerged more or less simultaneously with the first CCH publications in the early 1990s. As CCH researchers began to push beyond the boundaries of "traditional" epidemiology, they inevitably encroached upon other disciplinary boundaries, which lead to resentment:

The debate, although phrased in the muted and technical language of the scientific literature, turned into a slugfest. Much was at stake: professional reputations, the scientific consensus, and – most critical – societal consensus about the changes needed to deal with climate change (Epstein and Ferber, 2011: 51).

R20: There was sort of polarization, which was basically territorial I think … But I think they [the ecologists] were also offended that amateurs were moving into the area and trying to model the very simplistic models - which of course was inevitable.

R1: ... I suspect … that people just feel very annoyed that you've got outsiders who've come in and are, you know, taking the limelight.

On the other hand, scientists opposed to CCH research, principally ecologists, have sought to discredit the CCH community:

R4: … you will always in any field have your snake oil salesmen and the snake oil salesmen are very concerned to promote themselves rather than science.... And that quite honestly is a fact of life… I can think of a whole series of people who make their reputations on making predictions for health futures with very little experience of health in the present or in the past… And yet they've got an awful lot of publicity as a result of that. I think people like Jonathan Patz and Paul Epstein are the key players here; Pim Martens of course in the malaria field; and to some extent Tony McMichael in Australia.

The mutual distrust and resentment between the CCH community and its opponents is thus quite clear. The controversy has, periodically, received coverage in the scientific and popular media (Allen, 2011, Brower, 2001, Taubes, 1997), which offers a useful way of introducing some of its recurring themes. In 1997, for example *Science* published a news story entitled "Apocalypse Not" (Taubes, 1997). This article pits the CCH community, in this article referred to as "public health researchers", against the ecologists and their allies, referred to as "infectious disease specialists" (Ibid.: 1004). The infectious disease (ID) specialists react particularly strongly against the models produced by the CCH community, branding them as "'gloom and doom' speculations based on 'soft data' (1004)". Duane Gubler, then director of the division of vector-borne diseases at the US CDC, refers to the standard CCH doctrine as "gospel

(1004)" and argues that CCH models are "probably the most blatant disregard (1005)" for other factors driving disease spread.

The ID specialists cite a range of counter-arguments to discredit CCH models, including: the lack of conclusive evidence; the "simplistic (1005)" assumptions embedded in models of the impact of climate change on various mosquito-borne diseases; the inherent complexity in the dynamics of vector-borne disease transmission; and the abundance of alternative (and more proximate!) explanations for disease spread, such as microbial resistance to drugs, vector resistance to pesticides, the collapse of public health infrastructure, and demographics. As evidence of these factors, a natural experiment is put forward by Gubler, along with Paul Reiter, then also of CDC and one of the most divisive figures in the controversies surrounding CCH research.[2] Gubler and Reiter note that the incidence of dengue was much higher in Reynosa, Mexico than across the *Rio Grande* in Texas, thereby demonstrating the influence of non-climatic factors on dengue outbreaks: as both areas share the same climate, it must be their differing socio-economic contexts that account for the different rates of the disease.

The *Science* article also describes how the CCH community defends the design and purpose of their models. McMichael argues that the models are an important part of awareness-raising: they "serve notice…and there could be a range of consequences for human health (1004)". Epstein argues that Gubler overlooks worrying trends, such as the spread of mosquitoes to higher latitudes in concert with increasing temperatures, and McMichael argues that the

[2] Paul Reiter has been perhaps the most vocal opponent of CCH research, and he has also been vocal about his opposition to climate change science more generally. For example, in 2006 he was a co-signatory of an open letter to Canadian Prime Minister Stephen Harper urging him to reconsider his country's stance on climate change (see: http://www.canada.com/nationalpost/financialpost/story.html?id=3711460e-bd5a-475d-a6be-4db87559d605, accessed November 5, 2012). He has also been accused of receiving money from the anti-climate change oil lobby, but such claims are nearly impossible to verify. It does seem likely that he has spoken at conferences sponsored by the oil lobby, however. See, for example: http://www.exxonsecrets.org/html/personfactsheet.php?id=421, accessed October 28, 2012.

opponents to CCH are “mixing up the present with the future (1006)” by citing current or historical examples to contradict the models which are interested in the future.

The *Science* article finally notes the creation of various formal committees and panels set up to resolve the issue. The US CDC and National Research Council (NRC) established an expert panel to further examine the controversy, as did the Pan-American Health Organization (PAHO) and even NASA.[3] Despite such interventions, the controversy persisted. A 2001 story appearing in *EMBO Reports* offers further background (Brower, 2001). This article cites an essay published by Paul Epstein in *Scientific American* (Epstein, 2000) in which he discusses cases of malaria in the US, a resurgence of malaria in South Korea, South Africa, Europe and even the former Soviet Union, as well as the resurgence of dengue in the Americas and Australia: “While he acknowledges that these outbreaks could also be connected with a decline in mosquito control, he sees global warming as the most likely source for the current spread of vector-borne diseases (Brower, 2011: 755)”.

Similar to the *Science* article, the *EMBO Reports* article notes opposition to Epstein’s ideas: “the proponents of climate change’s impact on these diseases have not substantiated their claims with facts (Ibid.: 755)”; their work “grossly oversimplifies the issue (Ibid.: 755)”. Paul Reiter suggests that there have always been vector-borne diseases in temperate areas, stating: “it is unfortunate that public perception is distorted by people who know little about the field (Ibid: 756)”. Additionally, the point is made that even if climate change would lend some areas more amenable for disease spread, “human behavioural adaptations and public health interventions could mitigate many adverse impacts (Ibid.: 757)”. Along similar lines, CCH opponents can be seen in the

[3] Each of these panels offers further evidence of the “institutional hybrids” discussed in 3.3.3. The final findings from the NRC committee were published a few years later in 2001. The report concludes that modelling studies must be interpreted cautiously and that the potential disease impacts of climate change are uncertain, but it also calls for more (and ideally interdisciplinary) research and enhanced surveillance incorporating climatic and non-climatic factors (NRC, 2001, Epstein, 2000).

media coverage to be somewhat complacent when it comes to climate change. A *Washington Post* story from 2011 cites Duane Gubler as saying: "I think it makes more sense to use our money on research and public health than carbon trading (Allen, 2011)".

Thus some of the key battle-lines have been drawn in the climate-disease controversy – a battle that has by now been fought in many theatres: the peer-review literature, conferences and workshops, the popular media, the IPCC. Even a cursory examination of the highly quotable citations above reveals instances of boundary work. This was further elaborated in an analysis published in *EcoHealth*, a journal with close ties to the CCH community (Brisbois and Ali, 2010).[4] Brisbois and Harris Ali state that this article has its roots in "a graduate student's sense of unease with the extraordinarily heated exchanges … and with the apparent absence of important political considerations from both sides of the debate (Ibid.: 2)". In this study, the authors identify four "proponents" of CCH research relating to vector-borne diseases. These are Tony McMichael and Andrew Haines of LSHTM;[5] Paul Epstein from Harvard University; and Jonathan Patz from the University of Wisconsin. They also identify four CCH "opponents": David Rogers, Sarah Randolph and Simon Hay, all of the Department of Zoology at Oxford University, and Paul Reiter, as mentioned above, formerly of the US CDC and currently at the Pasteur Institute in Paris, France.

Brisbois and Harris Ali analyse the peer-reviewed literature authored by the proponents and opponents. The boundary work deployed by both "sides" of the controversy is identified. The inter-disciplinary nature of CCH research means that it is best "described as a process of challenging existing disciplinary frontiers (5; c.f. Frickel, 2004)". As the CCH community promotes their work

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[4] *EcoHealth* was created as a merger of two former journals, Ecosystem Health, and Global Change and Human Health. Currently, several members from the CCH community are involved with the journal. Howard Frumkin and Tony McMichael are associate editors, Simon Hales is a review editor, and Jan Semenza, Jonathan Patz and Pim Martens are Editorial Advisers. http://www.ecohealth.net/whoweare_editorialboard.php, accessed August 20, 2011.

[5] As noted earlier Tony McMichael eventually left LSHTM to return to Australia.

and the need for interdisciplinarity, their calls for pro-active action to address climate change establishes "the scientific consensus on climate science as a frame within which VBD dynamics are interpreted as a symptom of climate change (Brisbois and Ali, 2010: 5)". On the opposite side of the controversy, the authors note additional instances of CCH opponents describing CCH research as "naive" and "simplistic": "the language used by Reiter, Rogers, Randolph, and Hay repeatedly downplays the scientific credibility of those proposing climate-VBD (vector-borne disease) links (Ibid.: 6)".

This boundary dispute, they suggest, validates the idea that climate change is "serving as a collective action frame, within which VBDs are interpreted as climate change impacts, and interdisciplinary research is highlighted as the logical solution (Ibid.: 8)". Yet this, Brisbois and Harris Ali speculate, is also fuelling the controversy, for viewing VBDs as the products of climate change creates a leadership role for the CCH community that its opponents resent. Although this claim could have benefited from further substantiation, it is certainly plausible that the "opponents" would not willingly yield leadership in VBDs as a topic for research and public health action to the CCH community.

Also at stake, according to Brisbois and Harris Ali, is the appropriate "scale" of the issue. CCH proponents, they argue, tend to see the topic as a global problem, whereas CCH opponents tend to see it as one with local solutions. Additionally, consistent with the discussion from 1.4, Brisbois and Harris Ali, note that GCMs have led to the increasing global scaling of climate change and its impacts, and suggest the climate impact modelling community, whether health, disease, or otherwise, are "put into a lay position with respect to climate modelers…and a scientific hierarchy is said to have emerged with climate modelers at top (8)". CCH opponents, notably Rogers and Randolph, are documented as critiquing the validity and suitability of GCMs for modelling VBDs, and suggest instead that good analyses should "'pass through' the R_0 equation from infectious disease epidemiology (Ibid.: 9)". Yet, Brisbois and Harris Ali fail to note that CCH proponents also use the R_0 equation as a component in their models (4.3.1) or that they also view the solutions to CCH problems to be local (4.3). Identifying GCMs as relevant for this controversy is nonetheless an interesting starting point for further analysis as it begs the question: why would some scientists studying VBDs be more happy to work with GCMs than others?

The last part of their paper offers the vaguest of clues. In this section, it is suggested that both opponents and proponents are naive with respect to various aspects of globalisation. Paul Reiter, they suggest, is naive with regards to the inequalities that globalisation creates and "implicitly accepts its underlying 'neoliberal' orthodoxy (Brisbois & Harris Ali, 2010: 10)". Jonathan Patz and some of his co-authors, meanwhile, too willingly accept reductionist climate models, which over-emphasize climatic parameters and potentially normalise "the ethically problematic economic world order (Ibid.: 10)".

Essentially, Brisbois and Harris Ali claim that both CCH proponents and opponents are naive with respect to the power structures (with their roots in colonialism, etc.) that influence global health. Unfortunately, this argument, developed with the intent of ensuring that future iterations of this controversy are "more self-reflexive and politically aware (Ibid.: 11)", is neither substantiated nor harnessed to further explain the controversy. This is perhaps a consequence of their decision to limit the analysis to the peer-reviewed literature, but it is more principally related to their source of theoretical inspiration. Boundary work is useful for identifying and expressing the, well, boundaries surrounding a given controversy, but it is not particularly insightful with regards to its explanatory power. Similarly, the "collective action frames" concept as described in this essay does not offer analytical insight.

To summarise, Brisbois and Harris Ali successfully identify some of the key parameters driving the climate-VBD controversy and they offer a starting point for asking more in-depth questions. How and why CCH research has been conducted and contested? What are the related policy implications? What deeper political commitments are embedded in adopting climate change as a "collective action frame"? However, in neglecting the broader contexts shaping public health and ecologic research, Brisbois and Harris Ali have themselves developed an account demonstrating an "absence of important political considerations (Ibid.: 2)". As they confess: "it is virtually certain that there is more to the controversy than what we have been able to explain (Ibid.: 10)".

In order to analyse with greater perspective the manner in which the controversy has played out, it will be examined in detail as it specifically related to three VBDs, malaria (4.3 & 4.4), dengue (4.5), and tick-borne encephalitis (TBE) (4.6).

4.3 Debating climate-malaria futures: biological vs. statistical approaches

Research focused on climate change and its current and potential impacts on malaria transmission has been one of the key battle grounds of the climate-VBD controversy. A detailed examination of this research is essential for analysing it. In 4.3.1, the development of the systems (or biological) modelling approach to explore the relationships between climate change and malaria will be discussed. In 4.3.2, an alternative approach, statistical modelling, developed by some CCH "opponents", will be introduced while in 4.3.3, the key points of contention between these differing modelling approaches will be discussed.

4.3.1 The Biological (systems) approach: The Martens malaria models

In 1995, Pim Martens, Tony McMichael, and a few other co-authors published one of the early studies in the field, which presented a series of maps identifying future global risk areas for Malaria based upon various climate change scenarios (Martens et al., 1995b). The modelling work, as discussed in Chapter 3, was to a large extent designed to influence public health decision-making and to draw attention to climate change as an issue relevant to public health. The collaboration between McMichael and Martens was fuelled not only by a shared set of commitments but also by opportunism:

> **McMichael:** I think he saw that I was an unusual sort of ally because I was a senior figure in international epidemiology and somebody that … would be interesting and useful to work with. From my part it was a nice opportunity to actually find about scenario based modelling, which I had never done before.

This type of modelling, the authors asserted, "seems to be the only approach capable of adequately reflecting the complexity of the interrelationships between the climate system and mosquito and human population dynamics (Martens et al., 1995: 458)". Using this approach, Martens and co-authors sought to answer the question: "If other things were held constant in the world, what would be the impact of climate change *per se* on the distribution and incidence of malaria (Ibid.: 458)?" The project was ambitious, as it attempted to model the current and future global transmission of the two principal malaria parasites, *Plasmodium vivax* and *P. falciparum.*

The integrative modelling methodology is modular, with the outputs from one module serving as the inputs for the next one. The modules included a climate system, a malaria system (divided into a human subsystem and a mosquito subsystem), and an impact system. Climatic variables, principally temperature and precipitation, were said to impact the mosquito system. In turn, this system interacted with the human system to determine transmission rates, and the overall impact system calculates the public health impact of climate change on malaria.

The climate system

To model the climate system, the researchers employed a climatic dataset which used the standardised temperature and precipitation outputs from a GCM produced by the UK Meteorological Office. This GCM was based upon a climate change scenario that projected a temperature increase beyond the IPCC uncertainty range at the time (5.2°C as opposed to the IPCC range 1.5-4.5°C). The authors note that their "projected changes in malaria transmission will be more pronounced (Ibid.: 459)" than if the studies had used some other GCMs, but they suggest that the changes would not be overly significant.

The malaria system: R_0 and "epidemic potential"

In the Martens model, the malaria system is based upon R_0, the basic reproduction rate, essential component to much epidemiologic modelling of communicable diseases (e.g. Giesecke, 2001). This rate measures the number of secondary infections relating to one infection: "the average number of secondary infections produced when one infected individual is introduced into a host population where everyone is susceptible (Martens et al., 1995b: 459)". Epidemiologic modelling theory suggests that where $R_0 > 1$ an infection will continue to spread, but where $R_0 < 1$ it will die out.

In this study, the authors were particularly interested in the climate-sensitive components driving R_0 and focused on vector density in particular. Although they note that "it is impossible to estimate the change in vector abundance…as a result of temperature, precipitation and humidity changes (Ibid.: 459)", the authors suggest that R_0 allows for a calculation of the "critical density threshold of hosts necessary to retain parasite transmission (Ibid.: 459)". They therefore calculated malaria's "epidemic potential" as a proxy variable for vector abundance, which was defined as the reciprocal of the vector population's critical density. This was used to assess malaria risk under climate change scenarios:

> A high epidemic potential indicates that a smaller number of vectors or a less potent vector population may maintain a state of endemicity or give rise to occasional epidemics in a given area (Ibid.: 460).

In order to base modelling upon "epidemic potential", biological thresholds based upon temperature and precipitation were required as data inputs.[6] Based upon a literature review, the former took the form of survival probabilities for the mosquito vectors of malaria at different temperatures (9°, 20° and 40°C), and the latter was a minimum rainfall threshold required for mosquito survival

[6] Martens *et al.* (1995: 460) argue that "the distribution and population dynamics of malaria are probably more governed by abiotic than biotic factors", of which temperature and rainfall are the most important. These were therefore the two variables for which thresholds were sought.

and thus malaria transmission (an average of 1.5mm per day), thereby enabling the authors to exclude excessively dry regions from their model. Finally, although they concede that heavy rainfalls can flush out mosquito larvae and have a negative impact on malaria transmission, the authors do not incorporate a maximum precipitation level into the model.

The impact system

To model the human subsystem and overall impact system, the authors use UN population projections to assess the current and future populations in malaria risk-zones, according to two age classes, 0-4 years of age and 5 years and older. This population was then further stratified according to three categories from another classic epidemiological model: the susceptible-infected-immune epidemic model.[7] Once infected, the authors assume that individuals run a "standard (Martens et al., 1995b: 461)" risk of contracting the disease, since "the general level of prophylaxis is, *and probably will remain*, low in the populations concerned (Ibid.: 461; emphasis added)". Finally, to consider the differing levels of population immunity between endemic and non-endemic malaria regions, the authors also attempted to measure the overall health impact according to the disability-adjusted life years (DALY) metric.[8]

Conclusions from the systems models

Thus having established the model and its data sources and parameters, Martens *et al.* produced a series of maps for the baseline year 1990 and for the year 2100 under two different climate change scenarios, the IPCC business-as-

[7] The similar SIR (susceptible-infected-recovered) model suggests that the transition of patients between these three states ultimately influences and determines the course of an infectious disease outbreak (e.g. Cooke, 1979).

[8] DALY is a common measure used in health impact assessments or, more common in the public health discourse, the burden of disease. The basic premise is that the more severe a disease and the longer a patient suffers from it, the higher the DALY. However, aside from the many assumptions surrounding various epidemiologic data that are required to calculate DALYs, the usage of disability weights (0.6 for malaria in this study) and age weights requires assumptions about a "normal" population. Some controversy has surrounded this practice, such as that it has been observed to be value-laden. See, for example, Anand & Hanson (1997).

usual (BaU) and advanced policies (AP) scenarios.[9] The models for 2100 assess future risk by means of a ratio relating future "epidemic potential" to a baseline "epidemic potential" calculated for 1990. They argue that their 1990 models generally agree with the global distribution of malaria as it was "before the introduction of large-scale antimalaria campaigns (Ibid.: 461)". That is to say that the 1990 models for *P. Vivax* indicated potential malaria transmission in much of the United States, southern Russia, central Europe and Japan: endemic malaria was not present in these areas in 1990, but their assessment suggests that climatic factors were suitable for malaria transmission at that time. Thus, in their models, the authors have not accounted for public health measures or broader socio-economic factors that mitigate malaria transmission.[10]

The authors conclude that "an expansion of the geographic areas susceptible to malaria transmission and a widespread increase of potential malaria risk are to be expected as climate changes (Ibid.: 462)". Their analysis suggests that the epidemic potential of the mosquito populations that carry malaria could increase twofold in tropical regions and up to 100-fold in temperate regions. Noting that "the highest risks for the introduction of malaria remain in the nonendemic regions bordering on malarial areas (Ibid.: 462)", the authors highlight the high altitude areas such as East Africa or the Andes, where temperature increases could help currently nonmalarial regions become areas with seasonal epidemics. This conclusion, however, incorporates an assumption about the future ability of these areas to cope with climate change:

> Given that resources are insufficient to deal adequately with malaria in the most affected regions, increased risk of malaria due to climate change may seriously affect human health in the next century (Ibid.: 463).

[9] The advanced policy scenario refers to greater government policies towards reducing greenhouse gas emissions than the business-as-usual scenario.
[10] The authors do note that "the simulation of future risk areas must be interpreted to take account of local conditions and developments (Martens et al., 1995b: 461)" – but such conditions were not incorporated into their models or conclusions.

The authors also identify the temperate regions of Australia, the United States and southern Europe, arguing that there is a "real risk of reintroducing malaria into nonmalarial areas (Ibid.: 463)".

Refining the model methodologies and outputs

Martens and various constellations of co-authors would continue to reiterate and refine models presenting the relationships between malaria and climate change through the remainder of the 1990s, including one highly visible study published in 1999 (Martens et al., 1999). In this study, Martens and his collaborators, again including Tony McMichael, employed a more recent set of GCMs, the HadCM2 and HadCM3 models.[11] As with the previous Martens papers, the uncertainties and underlying assumptions behind these GCMs are not described; instead, readers are advised to seek the original publication describing them, which was published in the same supplemental issue of *Global Environmental Change* (Hulme et al., 1999).[12] Updated population projections were also used by Martens *et al.* in order to calculate the additional number of people at risk from malaria caused by the two principle parasites, *P. falciparum and P. vivax.*[13]

The authors claim that the new findings were based upon an "improved model (Martens et al., 1999: S90)". As in their previous model, this model explicitly did

[11] These models were designed to enable climate change impact modelling at several different time slices: a baseline period for 1990 (the mean 1961-1990 climate), 2020, 2050 and 2080. Martens *et al.* limit their description of the models to mentioning that four different simulations of the HadCM2 models were used, each "leading to four subtly different climate futures (S95)". The HadCM3 is described as being more recent than HadCM2, "un-flux-corrected (S95)" and, like HadCM2, a model that assumes "greenhouse-gas-only forcings (S95)". Four different HadCM2 simulations are used, as: "the range of difference between the ensemble members gives an estimate of the natural variability present within the forcing scenario".

[12] The HadCM2 and HadCM3 models were based upon the assumption of 1% per annum growth in greenhouse gas concentration, and the IPCC range was 0.5% - 1.1%. Hulme *et al.* (1999) write: "if a forcing scenario of only 0.5% per annum growth had been used to create our climate scenarios, the global warming would have been between 30 and 40% less than simulated for our scenarios (S16)". However, they also suggest that this would have been compensated by not considering the effects of sulphate aerosols in the modelling.

[13] These population projections were consistent with the projections that informed the HadCM2 and HadCM3 climate models. The projections were for total global population of 8.1 billion in 2020, 9.8 billion in 2050, and 10.7 billion in 2080. These were based on World Bank mid-range estimates of global population growth.

not model the future distribution of malaria's mosquito vectors under climate change scenarios, but in this one they did incorporate contemporary "continental-scale (Ibid.: S90)" estimates of the distribution of malaria's main vectors into the modelling. Other improvements that the authors point out are "species-specific relationships between temperature and transmission dynamics (Ibid.: S90)" and a "more realistic approach regarding malaria endemicity (Ibid.: S90)" for calculating future population risks of malaria. Perhaps most noteworthy is the replacement of the concept "epidemic potential" with "transmission potential". As the authors explained:

> The transmission potential of malaria is defined as the reciprocal of the vector density threshold. In previous assessments, TP was referred to as the epidemic potential (EP). However, this term (EP) did not correctly describe the epidemiology of malaria because higher malaria transmission intensity does not necessarily mean an increased risk of epidemics (Ibid.: S92).

As was also the case for epidemic potential, TP is calculated based a complicated formula incorporating a wide range of parameters taken from the published literature (from both field and laboratory experiments) such as the human biting rate by mosquitoes, human susceptibility to infection via mosquito bites, mosquito susceptibility to infection after biting an infectious person, daily survival probability of mosquitoes, the incubation period of the parasite inside the mosquito and the minimum and maximum temperature thresholds for such parasite development (Martens et al., 1999: Table 1).

As in the 1995 models, the authors also attempted to assess the potential future burden of malaria, based upon the relative change of TP from the baseline to the future climate scenarios and, this time, by assessing the additional population at risk of malaria under climate scenarios and according to the length of malaria transmission. The population at-risk was defined as the total population "living in an area where conditions are suitable for malaria transmission (Ibid.: S99)". This was despite a caveat that "not everyone classified as at risk is actually at risk (Ibid.: S99)" because the risk is ultimately modulated by socio-economic circumstances and different for those living in cities or rural areas. Estimates of the global distribution of malaria's mosquito vectors were used as a limiting factor for malaria transmission.[14] The authors calculated that roughly 300 million people would be at risk of *P. Falciparum* malaria in 2080 under the HadCM3 model (between 260 – 320 million with HadCM2 data), and 150 million (between 100-200 million with HadCM2) at risk of *P. vivax* malaria in 2080.[15] The results were summarised in a series of maps identifying the projected current and future global distribution limits of malaria (**Figure 2**).

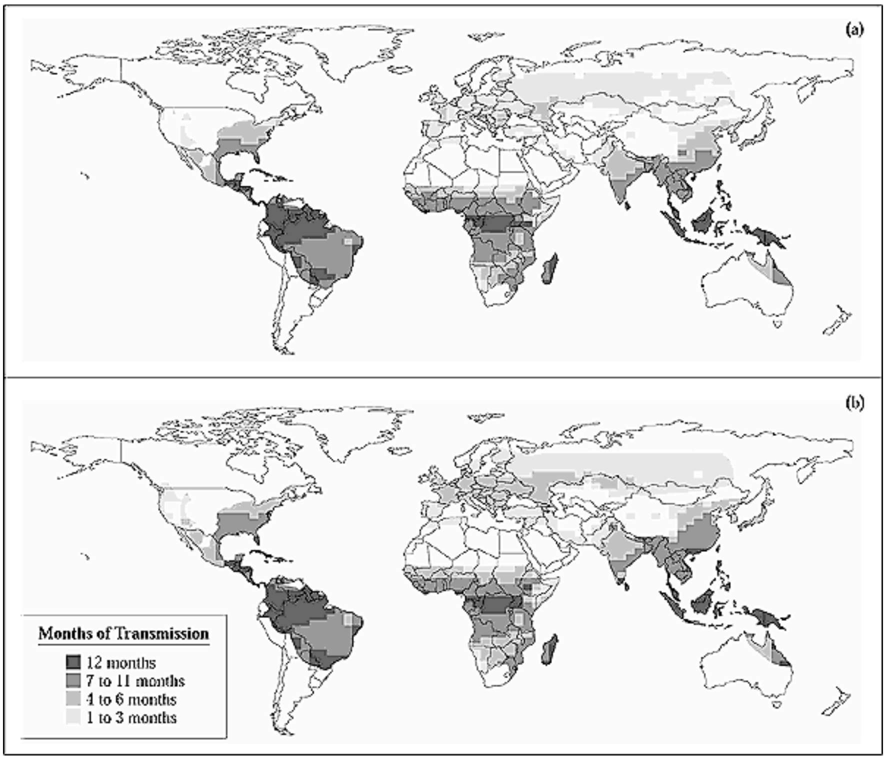

Figure 2. The Martens malaria models

The shading represents the number of months of malaria transmission, where a) is the time period 1961-1990 and b) is the 2080s.[16]

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[14] The authors note that previous estimates were higher where they did not limit the results by vector distribution: "Obviously, the additional number of people at malaria risk is higher when the absence of the vector is not considered to be a limitation for transmission (S102)".

[15] These populations were then divided into three risk categories. Areas with low TP value for three or less than three consecutive months per year were allotted to the "risk of epidemics" group; the "seasonal transmission" group was assigned to those where TP>0 for more than three but less than seven consecutive months per year,; and the "year-round transmission" group was assigned where TP>0 for seven or more consecutive months per year.

[16] The figure is available from IPCC. The caption for the figure, as presented in IPCC TAR (to be discussed in 5.3.3) reads as follows: "Potential impact of climate change on seasonal

Despite all of the modifications to the model, the overall conclusions do not dramatically differ from the earlier model:

> It is anticipated that climate change will affect the seasonal transmission and geographical distribution of malaria. At the borders of malaria transmission, the modelled changes in average length of the transmission season may be important (Martens et al., 1999: S103).

This conclusion, accompanied with the disclaimer that findings are not "predictions of the future (Ibid.: S105)" but rather "trajectories of possible changes in malaria risk (Ibid.: S105)", once again identifies two principal "risk zones" for future malaria transmission. One is temperate areas, including Europe, Australia and the United States during the summer months, where they note that the vector is present, the climatic conditions are permissive of malaria transmission, and the frequency with which malaria-infected travellers visit or return to these areas. Although they argue that these areas are unlikely to turn endemic, they argue that climate change could increase the risk of sporadic malaria transmission. They furthermore note that Azerbaijan, Tajikistan and Turkey returned to a state of malaria endemicity coinciding with a decline in public health infrastructures, which "illustrates the vulnerability of these (temperate) regions (Ibid.: S105)".

High-altitude areas, particularly in east Africa, again constitute the other key risk area. Claiming that malaria had been a growing problem in the African highlands in recent years, the authors note that this was "probably due to a decline in the control and treatment of malaria (Ibid.: S104)" but provide a central role for climate change in their account:

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transmission of falciparum malaria. Output from MIASMA v2.0 malaria model (Martens et al., 1999) indicates the number of months per year when climate conditions are suitable for falciparum transmission and where there is competent mosquito vector: (a) months of potential transmission under current climate (1961-1990); (b) months of potential transmission under a GHG-only climate scenario (HadCM2 ensemble mean) in the 2080s. Future changes in mosquito distributions are not modelled. This model does not take into account control or eradication activities that have significantly limited the distribution of malaria". Available at: http://www.grida.no/publications/other/ipcc_tar/, accessed February 28, 2012.

> Whilst inadequate health care makes communities vulnerable to malaria, the factors which precipitate epidemics are often climatic in origin, including sharp increases in rainfall, temperature and humidity. It seems likely that epidemic prone areas are those which experience marked differences in interannual climate and where the thresholds required for malaria transmission are exceeded every few years (Ibid.: S105).

The authors concede that their models did not account for future adaptive measures that risk areas might undertake, which could prevent malaria from becoming a major problem, but as concerns eastern Africa particularly, they argue that "many of the current technical, socio-economic and political barriers to successful prevention and control *will also apply in the future* (Ibid.: S105; emphasis added)".

Summary: On the legacy of the Martens malaria models

The production of the malaria models described above involves a wide range of complicated mathematical formulas and data inputs. An impressive number of choices need to be made in producing these models, including: which climate change models and scenarios to use; whether to model at the regional, continental or global level; which population projections to use; whether to model R_0 directly or to develop a formula for epidemic or transmission potential; which biological parameters to use to inform the calculation of the transmission or epidemic potential; whether or not to include projections of the distribution of vectors as a model input; how to classify a population at-risk; whether to model future adaptive measures or public health capacities; and so on.

Many of these choices have a great deal of inherent uncertainties surrounding them. There are, additionally, many value-laden judgments, notably about the respective ability of nations to tackle malaria in the future, based upon their socioeconomic circumstances and future public health capabilities, in addition to the many socio-economic assumptions already incorporated in GCMs (1.4). Consequently, the malaria models described above are as much a product of the judgements and commitments of the scientists that created them as they are a product of mathematical or biological reasoning. In their production and uptake, climate-malaria models stabilise and are stabilised by these underlying judgements and commitments, producing one set of climate-malaria futures at the expense of potentially different ones.

Just as the Martens malaria models outputs were being incorporated into the 3rd IPCC Assessment Report in 2001 (to be further discussed in 5.3.3), thereby gaining an even higher degree of visibility, a group of ecologists from Oxford University had started to worry that the Martens models were fast becoming not only the orthodox set of climate-malaria futures but of climate-health futures more generally. They began to resent this attention and decided to do their own set of models:

> **Jonathan:** Was this [the popularity of the Martens model] part of the reason why you embarked on the projects…?
>
> **R4:** Absolutely… for about the five years before that we were getting increasingly irritated about Pim Martens producing these models and quite frankly reproducing them multiple times. So he got lots and lots of publications all saying the same thing in multiple journals and the more these came out the more frustrated we got.

One of their main concerns was that the vision of the future embedded in the Martens models would become dominant:

> **R17:** … the papers that were published [by Martens *et al.*] predicting the spread of malaria, predicting those predicting maps that were based, we would argue, on the wrong method...And they were very dramatic and they were repeated in many, many publications … they have left a lasting legacy of a mindset…that is hard to shake.

Shaking this mindset, however, is exactly what they attempted to do.

4.3.2 Statistical approaches: The Rogers/Randolph models

One of the most visible and influential rebukes of CCH research was published by David Rogers and Sarah Randolph of Oxford University in the journal *Science* (Rogers and Randolph, 2000). The Martens malaria maps, they argued, produced "noticeable mismatches (Ibid.: 1763)" to known malaria distribution, including false-positives in the United States as well as false predictions of absence. They suggested this was because these models could not adequately account for vector abundance: "The trouble…is the single biggest variable is in fact vector abundance, and it's the one thing we have no model on (**R17**)". Thus malaria cannot be modelled "satisfactorily because crucial parameters and their relations with environmental factors have not yet been quantified (Rogers and Randolph, 2000: 1763)".

Another important critique focused on the use of R_0 in malaria modelling. Rogers and Randolph (2000) argue that when predicting malaria transmission based upon R_0, "absolute, not relative, estimates of all quantities in the equation are needed (Ibid.: 1763)". This is a point that CCH opponents would raise often:

> **R4:** … R_0 has got to be above one which means that one case at the present time gives rise to more than one case in the future. That is the condition for malaria to exist anywhere in the present and the future...
>
> … all that Martens' models are saying is in a globally warm future each malaria case will give rise to 0.1 cases in the future rather than 0.01 cases or 0.001 cases. But … the R_0 of 0.1 … is still below the threshold of 1. So however much the proportionate increase is it doesn't increase above the absolute value of 1 and therefore there will be no malaria in arctic Russia until the absolute value is above 1 ... And Martens completely missed this in his original papers – or he chose to ignore it – and it generated a lot of publicity to say that so many tens of millions of people would be at risk of malaria in the future that aren't at the moment, including many arctic Russians. And all that was complete nonsense.

Following this logic, comparing present with future ratios of epidemic potential or transmission potential to assess future malarial transmission is "an inappropriate measure of changing risk because a high ratio may still leave

$R_0 < 0$ (Ibid.: 1763)". In some areas where Martens had projected an increase in risk, the Oxford researchers would have projected the opposite.

The statistical modelling methodology

The models produced by Rogers and Randolph (2000) are based upon a statistical modelling approach, as opposed to the biological/systems approach followed by Martens and his collaborators. Statistical modelling involves modelling the then present-day global malaria distribution to assess the "climatic constraints (Ibid.: 1764)" influencing the global transmission of malaria. The results were then used in combination with a few different scenarios from the HadCM2 GCMs (also used by the biological modellers) to predict the future distribution of malaria.

The Rogers-Randolph malaria models were based upon the minimum, mean and maximum values from three climate variables: temperature, precipitation and humidity (vapour pressure). A complicated set of procedures modelled random selections of 1500 data points inside ("presence") and 1500 data points outside ("absence") of malaria's recorded geographical distribution with Fourier-processed climate data.[17] The result is a series of data clusters for the presence and absence points; the authors suggest that "clustering essentially allows for nonlinear responses of biological systems to gradual changes in climatic variables (Rogers & Randolph, 2000: note 16)". The data clusters then underwent discriminant analysis to identify the climatic conditions most significant for determining malaria presence and absence.[18] These variables were then used to map globally the predicted presence and absence of malaria based on current and future climate.

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[17] Fourier processing of global climate data involves "smoothing" the data from a long time-series so that specific variables, such as mean minimum or maximum temperature, can be extracted and applied to statistical modelling. The method has been described in detail by Rogers, Hay & Packer (1996).

[18] The approach adopted by David Rogers has evolved into non-linear discriminant analysis. In both approaches, the assumption is that variables consistently influence the presence of absence of the outcome (e.g. of vectors, or of disease). For a review of the topic see Rogers (2006).

Modest returns: findings from the statistical models

Rogers and Randolph (2000: 1765) report that "only a relatively small extension" of malaria would occur in the future. Under the HadCM2 medium-high scenario, 23 million additional people would be at-risk, but under the HadCM2 high scenario, 25 million fewer people would be at-risk globally. These changes, they suggest, "are modest because covariates limit potential expansion along certain dimensions of environmental space (Ibid.: 1765)".

As concerns co-variation, Rogers and Randolph (2000) argue that, from a biological perspective, organisms can only cope with the extremes of some climatic variables if the other variables influencing it are not extreme. They provide a hypothetical example: models based upon only minimum mean temperatures would predict a significant expansion of malaria northward into the Sahara, as the cold desert nights would not be prohibitive of mosquito and malaria development, but a multivariate model does not predict such an expansion because of the influence of the other key variables, rainfall and humidity. Thus, their quantitative malaria model:

> highlights the use of multivariate rather than univariate constraints…and the advantage of statistical rather than univariate approaches in situations where biological knowledge is incomplete (Ibid.: 1765).

Thus differentiating the statistical modelling approach from the biological one, Rogers and Randolph proceed to critique the practice of modelling using climate change models. More generally, they argue that, regardless of the modelling approach used, modelling with GCMs is problematic due to: low spatial resolution; local variations; the way in which they represent "mean conditions across large geographical areas that may not occur in many places within them (Ibid.: 1765)"; and the unknown accuracy of GCMs for predicting covariating climate variables.

One final item of interest in the Rogers/Randolph models is the way in which they implicitly incorporate measures of "good" public health control. Their work was based upon present-day maps of the distribution of malaria, which included climatic areas known to be climatically amenable to malaria transmission but that no longer experience endemic malaria transmission due to public health control measures. One example is the southern United States, which is described as an "absence" point in the Rogers/Randolph models. The significance of this is that factors beyond the climatic variables used as model inputs play a role in determining "absence" (and therefore also "presence"), thus influencing model outputs. Rogers and Randolph, intriguingly, argue that climate continues to play a role in such regions: "In cooler regions, where mosquito life-spans barely exceed extrinsic incubation periods, transmission cycles are inherently more fragile (Ibid.: 1764)". In other words, "along the edges (Ibid.: 1764)" of its distribution range, climate helps to determine whether public health control measures are successful. Regardless of the validity of this argument, it is important to note that this is, effectively, the inverse of the assumptions that Martens and his co-authors made when they modelled "transmission potential" based on biologic parameters without incorporating measures of public health capacity. It is likely no accident that such modelling decisions have aligned with attempts to either prove or disprove the role for climate change in malaria transmission.

4.3.3 A constant controversy

Both statistical and biologic/systems models are impossible to definitively prove or falsify, given their future orientation. As such, the credibility stakes have become quite high – actors on both sides of the controversy seem to realise that scientific arguments alone will not settle it. Ever newer models are presented, but as soon as they are published a new round of scrutiny begins. Paul Epstein, Andy Haines and Paul Reiter can be seen thrashing it out in the letters of *Lancet* (Epstein et al., 1998b), for example, while Pim Martens and Paul Reiter have exchanged pleasantries in the letters of *Emerging Infectious Diseases* (Martens, 2000, Reiter, 2000b). After an updated climate-malaria model was published in 2003 (Tanser et al., 2003), its scale (continental Africa), its use of a "parasite ratio", the decision not to use population projections but rather 1990 population levels to calculate future populations-at-risk, and, unsurprisingly, its conclusions all came under attack (Reiter et al., 2004).

Accusations and hostilities have also coloured this literature. Critics of the Tanser et al. (2003) model stated that the study did not adhere to "the classical components of science – unbiased observation and systematic experimentation (Ibid*.:* 323)". The proponents, in reply, have defended their right to conduct such research: "it is not necessary to be a malariologist (Hales and Woodward, 2005: 258)" to appreciate the importance of climate on malaria. When it comes to climate change, society cannot afford "to postpone policy decisions until the likely outcomes are clearer, since to do so risks serious and potentially irreversible effects (Ibid.: 258)". The opponents, expectedly, disagree: the simplicity and uncertainties inherent in future-modelling are "precisely the reason why we should be so cautious (Thomas and Hay, 2005)".

Today, more than fifteen years after the publication of the early Martens models, the controversy over climate-malaria futures continues. New iterations of biologic, statistical and other types of models are continually produced (e.g. Beguin et al., 2011) and yet, despite this and despite attention to the others' critiques, both CCH proponents and opponents continue to come to conclusions strikingly similar to the much earlier models (e.g. Gething et al., 2010, Parham and Michael, 2010). It has been, and is very likely to continue to be, a war of attrition.

To understand why, it is instructive to identify some of the ways in which deeper disciplinary and political commitments have driven the climate-VBD debate.

4.3.4. Factors underpinning the climate-malaria controversy

The biological and statistical modelling approaches discussed above (4.3.1, 4.3.2) are based upon different methodologies and lead to very different conclusions as concerns both the current and future global transmission of malaria. Each modelling approach tacitly endorses specific scientific-politic visions, whether ones embedded in GCMs, global population projections, or future adaptive and public health capacities in both developing and developed worlds.

Socioeconomic visions of the future

Following the publication of Rogers and Randolph (2000), some members of the CCH community responded via a letter to *Science* (Martens et al., 2000; Appendix 1).[19] Although it was ultimately not accepted for publication, it is representative of arguments that have been used elsewhere. The letter, which defends biological modelling as a "legitimate scientific exercise", argues that biological and statistical approaches cannot be compared as they are estimating different parameters and asking different questions. The authors "question the analytical approach" adopted by Rogers and Randolph and note that, "on a global scale, all current biologically based models show net increases in the transmission zone of malaria and changes in seasonal transmission" under various climate change scenarios.

The authors pick up on the way in which the Rogers/Randolph statistical models tacitly incorporate socioeconomic advances into their models. They argue that the Rogers/Randolph model:

> …assumes that those contextual factors will apply in future in unchanged fashion. This adds an important, though speculative, element of multivariate realism to the modelling - but the model thereby addresses a qualitatively different question from the biological model.

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[19] The letter was provided through personal correspondence after following-up on an interview. The full letter is presented in Appendix 1.

By incorporating such factors, they continue, Rogers/Randolph forego "much information on the malaria/climate relationship within the temperate-zone climatic range", an area, it is suggested, that will be "considerably important" for malaria transmission in the future.

This is a particularly salient aspect of the debate and it deserves repetition: CCH proponents and opponents alike embed judgements about future socio-economic development into their work, but to different ends. Rogers and Randolph, for example, claim to be modelling malaria's "climatic constraints" even though, as they concede, socio-economic factors are embedded in their models. The hidden assumption is that temperate areas will have every opportunity to thwart malaria in the future, not only because its transmission is "fragile", but also because they are currently wealthy. One consequence is a likely bias in their definition of "presence" and "absence" points for malaria. Because rich temperate areas like northern Australia or the southern USA are currently free from malaria, all temperate areas of the world (currently, and in future projections) are more likely to register as "absence", regardless of their socioeconomic context. In this way, Rogers and Randolph prioritise the present over the future and socio-economic factors alongside climatic ones, even if they claim to only be modelling the latter. The consequence is a more conservative estimate of the potential impact of climate change on malaria transmission.

Conversely, Martens and his co-authors have as a central emphasis of their model a formula for "transmission potential" that is based upon biological variables that influence malaria transmission. As such, their model pays much less attention to present-day malaria than does the Rogers/Randolph model. In this way, they prioritise the future over the present and climatic over socio-economic factors. They do, however, consider socio-economic influences on malaria transmission in the way they interpret results. When they discuss malaria and the east African highlands, for example, they assume that it is likely that socioeconomic factors impeding malaria control will continue to be a problem in the future (4.3.1). As concerns rich, temperate countries, they argue that they should be vigilant to the increased malaria risk driven by climate change: should socioeconomic conditions or malaria or vector control programmes deteriorate, those countries could suffer from malaria just as it returned to Tajikistan and Azerbaijan in the 1990s.

Thus, the CCH proponents, consistent with their community's commitments and context (Chapter 3), argue that malaria, driven by climate change in the future, will be an increasing problem. It may be somewhat modulated by socio-economic circumstances, but these can't be expected to be better in the future than they are today. Mirroring the CCH community's more holistic approach to public health, they tend to view globalisation and its uneven economic consequences as an exacerbating factor: "Human development has many goals, one of which is to protect human health… Unfortunately, we often fail to attain this goal (Woodward et al., 2000: 1148)".

Meanwhile, CCH opponents argue that malaria, somewhat modulated by climate change, will not be that much more or less of an issue in the future than it is today. Instead, socio-economic circumstances will continue to substantially modulate malaria transmission. As opponents like Paul Reiter have repeatedly argued, rich temperate countries have historically suffered from malaria but eradicated it through improved living standards: "unless living conditions are drastically changed, global warming is unlikely to give rise to major epidemics of tropical mosquito-borne disease in the USA (Reiter, 1996)" (or in other similar countries).

Claims on expertise and climate science

The future-orientated nature of climate-disease modelling means not only that socioeconomic and climatic visions of the future are incorporated (explicitly or implicitly) into models, but also that model outputs cannot be definitively proved or disproved. This serves to shift attention to the producers of the models rather than the models themselves, perhaps explaining the tendency towards hostility that this controversy displays.

As one CCH opponent suggested – doubtless related to their prominent recognition as an "infectious disease expert" – because future models cannot be verified, the track record of the scientist producing the future models should be heavily scrutinised:

> **R4:** … if you want to judge … the conflicting views of scientist X and scientist Y's predictions about the future you go back and look at their track record in other areas of science… Let's say scientist Y never actually worked on malaria at the present time and has always predicted malaria in the future. When looking at what scientist Y says about malaria in the future you can't really judge how good that is because he's said nothing about malaria at the present time. And so his predictions could be completely oddball and completely wrong and there's no yardstick for us or anybody else to judge the worthiness of his science…

Assessing the previous track record of a scientist is, of course, contentious. Clearly, the CCH opponents, for their part, do not consider the CCH community's science to be particularly "worthy":

> **R13:** what shocked me really was that they talked, well they talk less and less now, about climate change and vector borne disease with authority, although they had never actually produced any research papers in that field….

> **R17:** … people who were producing them [systems models] were not actually biologists. I know because I've had conversations with one or two of them at meetings….they were not familiar with the complexity of the system, and how things could go very wrong if you just apply formulae without thinking about what would underpin those formulae.

Consistent with this argument, Rogers and Randolph (2000: 1763), state that until malaria models "can give accurate descriptions of the current situation of global malaria, they cannot be used to give reliable predictions about the future". Thus models that predict present-day malaria in areas such as the US or Australia should not be trusted to make future predictions. The *right* answer, rooted in the *right* expertise and methodology, they suggest, is the outcome of a statistical, rather than biological, approach which:

gave a better description of the present global distribution of *P. falciparum* malaria and predicted remarkably few future changes, even under the most extreme scenarios of climate change (Rogers and Randolph, 2000: 1763-4).

It is, of course, not very surprising for the more established researchers in a field to resent, and seek to police, the boundaries of their discipline. Their efforts, and the efforts of the CCH proponents in attempting to assert their right to work in the area, demonstrate the high stakes involved in being able to claim to be *the* expert in a growing field.

Thus CCH opponents sought to downplay the attention that models like the Martens malaria models drew by attempting to redirect attention back to the present:

R4: And I think very few ... global warming people bother to think about opportunity costs of what they're asking for. And I think we should do….

…what we really are worried about is the fairly scarce resources that we've got being thrown in all the wrong directions to meet threats that may never actually arrive.

Paul Reiter: Public concern should focus on ways to deal with the realities of malaria transmission, rather than on the weather (Reiter, 2000a: 10)

R13: it's very, very complicated, the whole thing … and of course, there are different people within the system who have different biases or different goals as well. We have among us a person that we all rather joke about, who is very committed towards the sort of catastrophic side, and he is a modeller, so he can produce catastrophic models as much as he wants.

On the other hand, CCH proponents have counter-claimed that the CCH opponents are simply climate change sceptics:

R20… people like David Rogers and Sarah Randolph from Oxford were clearly sceptical about the climate change process itself… Paul Reiter…has always been very hostile to the idea that climate is related to these diseases ... I suspect that he actually belongs to the sort of fringe sceptical or denier group.

> **R19:** ...malaria for example is a highly climate sensitive disease. And I don't think anyone would dispute that. It is climate sensitive. The astonishing thing to me is that the Oxford Group have managed to produce paper after paper that show that malaria is not affected by this, that or the other change in climate. And I think they've had to stretch reality quite vigorously in order to achieve that.

Thus some of the key fault-lines of the climate-VBD controversy can be seen, and they will re-emerge in the specific debates surrounding dengue (4.5) and tick-borne encephalitis (4.6). Beforehand, however, a controversy surrounding highland malaria (4.4) will examine how the tensions in the debate have been further exacerbated by the reliance upon climate data for climate-disease modelling.

4.4 Highland malaria: the case of changing climate data

In 4.3.1 it was noted that the biological modelling conducted by Martens, McMichael and their collaborators drew attention to the highland areas of east Africa as one region particularly vulnerable to malaria, particularly under climate change scenarios. In fact, in the late 1990's Martens and a co-author developed a climate change model for this region and concluded that their projections "demonstrate that rises in temperature are likely to increase the risk of epidemics in the highlands both on continental and national scales (Lindsay and Martens, 1998: 42)".

Highland malaria would become a focus for CCH research aimed not only at projecting future climate changes but also at demonstrating that the health impacts of climate change had already been occurring (so-called "fingerprint" studies). This is because, as Paul Epstein has suggested, conducting research in a mountainous region enables one to switch between different climatic regions relatively easily; shifting only four meters on a mountain roughly corresponds to the same temperature change as shifting 2.4 kilometres in latitude (Epstein and Ferber, 2010: 48). Mountain regions are thus "perfect petri dishes (Ibid.)"

for assessing a range of climate change impacts, including its impact on malaria transmission.

In the 1990s, Andrew Githeko, a malaria researcher interested in the impacts of climate change in Kenya, observed that malaria epidemics had spread from 3 to 13 districts in western Kenya since 1988, and that monthly maximum temperatures in the region had increased by 2 °C during the same time period (Githeko et al., 2000). At altitudes of 2000m, the mean monthly temperature was at the biologic threshold for *P. Falciparum* malaria transmission, leading him to conclude that "further warming should affect areas above 2000m in east Africa (Ibid.: 1138)". This conclusion was consistent with other CCH community research, such as the aforementioned paper by Lindsay and Martens (1998) as well as one authored by Paul Epstein and Pim Martens (Epstein et al., 1998a).

From the onset, their conclusions were strongly contested. Paul Reiter quickly countered that an examination of the historical presence of malaria across much of the Western world argues for the importance of living standards and hygiene, instead of climate, on the transmission and eradication of vector-borne disease (Reiter, 2001). A group from Oxford, led by Simon Hay and including David Rogers and Sarah Randolph, published a model with their own findings. In a paper published in *Nature*, they examined the long-term meteorological data for four high-altitude East African locations (one each in Kenya, Uganda, Rwanda and Burundi) and concluded that the climate had not significantly changed over the past century. Given this, factors other than climate must have been responsible;[20] therefore there is "no need to invoke climate change (Hay et al., 2002a: 908)". Hay and co-authors followed this analysis up with a literature review entitled "*Hot topic or hot air?*" which they produced with the objective to "focus on the real and immediate causes of…malarial resurgences (Hay et al., 2002b: 533)". They argue that although Rogers and Randolph (2000) anticipated some future impact (albeit marginal) from climate change on

[20] As they had cited previously, examples of alternative explanations included the rise of antimalarial drug resistance, breakdowns in public health and population migration.

malaria spread, it is "quite another matter to attribute recent resurgences (Hay et al., 2002b: 530)" in highland malaria to climate change. They conclude by noting that "reversing, or even delaying…global climate change will not address the problems faced by Africa at risk from malaria today (Ibid.: 533)".

This intervention from Oxford led to a "very, very unpleasant spat **(R4)**" with the CCH community. As discussed in 4.3, the debate over future climate models largely focused on the choice of methodology, and underlying assumptions about the importance of socioeconomic development. Yet a striking feature of the "highland malaria spat" is its strong focus on the quality and relevance of the climatic data used in analyses. Several members of the CCH community, including Jonathan Patz, Andrew Githeko and Tony McMichael, published a reply in *Nature* in collaboration with Michael Hulme, who had been very active in the boundaries between climate change model producers and users.[21] In this letter, they focus most of their objection to the Oxford team's findings by arguing that the conclusions are likely "flawed by their inappropriate use of a climate data set (Patz et al., 2002: 628)". The Oxford group had used downscaled gridded climate data to interpolate climatic trends at the specific study sites, but the CCH proponents argue that this data is only appropriate for up-scaling to African regions, not "down-scaling to specific area locations (Ibid.: 627)", because interpolations ignore the local elevations. The authors note that the mean altitudes used by Hay *et al.* differed by an average of 575m from the input weather station sites, corresponding to a temperature deviation of 3 °C. Thus, the CCH proponents conclude, climate and malaria data sets "must be considered at comparable spatial and temporal scales (Ibid.: 628)". Where such research has been appropriately conducted, the authors suggested, they found "a close association (Ibid.: 628)" between malaria transmission and monthly temperature trends between 1997 and 2000. Finally, the authors of this letter also argue that even in the theoretical absence of a "historical climate signal

[21] Hulme was at the Hadley Centre, which produced the HadCM2 and HadCM3 datasets used in the Martens models, see 4.3.1. He has also worked extensively with climate change impacts communities, and, more recently, ahas also explored the social and normative dimensions of climate change in addition to scientific ones (see Hulme, 2009).

(Ibid.: 627)", this does not negate the validity of forward-looking predictions: even "non-significant" climate changes could influence malaria transmission should they influence critical climatic thresholds governing malaria transmission.

In their reply to this letter, Hay and his co-authors note that they did not focus on the future situation of malaria transmission, shifting the attention back to present-day malaria. They furthermore claim that the climatic datasets they used were indeed appropriate; the procedure used to generate the climate data takes account of altitude, and that the dataset has been used to quantify climate change across Africa: "it is inconsistent to assert that these same data are insufficient to demonstrate a lack of climate change (Hay et al., 2002c: 628)". They furthermore stress that "the purported warming trend is not significant (Ibid.: 628)"; as little is known about "non-significant" climate change and its impact on malaria, claims surrounding this cannot be substantiated. Thus: "evidence against the epidemiological significance of climate change in the recent malaria resurgences in Africa is mounting (Ibid.: 628)".

A shifting debate: moving beyond "core" expertise

The debate over climate change and its links to malaria evolved so as to have a much broader scope, by also considering whether or not climate change was occurring and how and which climate datasets should be used in analyses. It is a debate that would continue along the same lines for several years, with new publications weighing in to support both perspectives (e.g. Hay et al., 2005, Zhou et al., 2004). Similar to the debate over climate-malaria futures (4.3), there has been no resolution to this controversy:

> **R4**: Hay's paper has been incredibly controversial…and it was partly due to the fact that they were using quite different climate data sets analysed in slightly different ways and certainly over slightly different time periods. And we would stick by our original conclusions and the other side would stick by their original conclusions.

This respondent continued their assessment of the controversy by focusing on the implications of working with data produced by the global climate change community, who "know the inadequacies of their models (**R4**)". Yet these:

> **R4**: … tend to get ignored by the communities that adopt those predictions… so the health community took global climate futures or global climate interpolations as God-given truths… and they used them to make future predictions.

CCH proponents, meanwhile, have made similar claims against the CCH opponents. Andrew Githeko, for example, suggested that Hay and his colleagues' analysis was based upon "statistical smoke and mirrors (Epstein and Ferber, 2010: 52)".

Prediction and trust: the uncertainty cascade

The highland malaria spat demonstrates how the broader controversy surrounding CCH research can end up focusing on climate change science. There has been much debate about predictive value that CCH opponents and proponents assign to their climate-disease models. Are model outputs predictions, warnings or false alarms? In this regard, it is interesting that CCH proponents and their nearest policy communities do appear to treat the results from climate-malaria models as being face-value in the policy world (see 4.7). Yet in the more technical spheres, where CCH opponents have cried foul, CCH proponents have tended to modulate their claims:

> **R1:** …in our papers have been trying to say, "Hang on, we're not claiming to predict, we're not claiming prediction". That's beyond our capacity or anyone's. What we're saying is that if everything else remained constant, then the changes in the climate will lead to a … greater potential for disease to spread … in some parts of the world.

Regardless of the "true" predictive value that climate-disease models have, the debate over the underlying GCM data appears to support the idea of an "uncertainty cascade" through which uncertainties get washed over as they move from producers to users of models. Just as policy worlds are reliant upon trusting that CCH proponents' (or opponents') results are valid, CCH researchers themselves are reliant upon the producers of GCMs. In the highland malaria controversy, it is noteworthy, for example, that there is only one "pure" climate modeller involved, who joins sides with the CCH proponents to discredit the opponents' assessment of climate data. This is unsurprising. Clearly, it is in the self-interest of the GCM modelling community to see their models gain wider usage. As David Demeritt has noted: "the application of upstream outputs from state-of-the-art GCM outputs by impact-assessment experts enhances the credibility of their own work (Demeritt, 2001: 320)". The reproduction of GCMs in "impacts" research, such as climate-malaria modelling, further stabilises the assumptions and worldviews embedded within GCMs, a point previously discussed in 1.4.

The flip-side to the issue is that as GCMs, themselves inherently uncertain, are applied to model additional layers of complexity, it becomes easier for a community of opponents – for whatever its reasons – to cast doubt on the model outputs. As seen in 4.3, objections can be focused on the methodologies and assumptions related to impact modelling itself, or, as discussed here, they can focus on the underpinning climate data. As there is no scientific method for resolving these objections, the potential always exists that they end up in a seemingly endless technical debate about climate or climate change itself, in addition to (or rather than) a debate about the *impact* of climate or climate change on their topic of interest. Where this happens, the debate moves further afield from the "core" expertise of the impacts researchers, making both sides of the debate even more dependent on those with GCM modelling expertise.

Ultimately, researchers conducting climate impacts research need to choose whether to trust GCMs enough to work with them. As we have seen here, this decision is heavily dependent upon how their disciplinary and/or political interests align with the outputs from the final impacts models.

4.5 Modelling Dengue: “There’s no place like home”

Climate-disease research has also focused on dengue, another mosquito-borne disease. Dengue can occur as dengue fever, or dengue haemorrhagic fever, with the latter being common but more lethal. Dengue is primarily spread by the mosquito species *aedes aegypti*, but a secondary mosquito vector, *aedes albopictus*, is capable of transmitting the disease. Dengue incidence globally has increased dramatically over the past fifty years and, simultaneously, *aedes albopictus* has spread to new locations, including the United States and Mediterranean Europe, sparking fears that dengue could now also be transmitted in these areas. In this section, attempts by the CCH community to model dengue using statistical approaches will be investigated.

The vapour-pressure model

As the controversy surrounding climate change and vector-borne diseases progressed, it fuelled methodological refinements as both sides attempted to definitively “win” the controversy. It was perhaps inevitable, then, that methodological approaches (but not conclusions) managed to cross enemy lines. With the objective of providing “vital information for policy makers who want to understand the potential effects of climate change (Hales et al., 2002: 833)”, a team of CCH proponents modelled dengue based upon the statistical approach:

> **Hales:** I guess the idea for it really came from the paper in *Science* by Rogers and Randolph ... which was a spatial model of the distribution of malaria.

The paper, published in *Lancet*, attracted significant attention. Perhaps anticipating resistance to their research, the authors present the project rationale and allude to some of their underlying assumptions about future public health capacity:

> Climate is one of the fundamental forces behind epidemics, and its effects become evident if adaptive measures falter or cannot be extended to all populations at risk (Ibid.: 830).

To produce the model, Hales *et al.* plotted the presence and absence of known Dengue fever outbreaks globally between 1975 and 1996 alongside monthly averages of various climatic factors: rainfall, vapour pressure, and minimum, mean and maximum temperature. These variables were assessed for their significance, both singly and in combination with each other, via logistic regression to assess their relative importance in driving the risk of Dengue. The authors concluded that vapour pressure (i.e. humidity) was the most important predictor of dengue, and then produced a final "cautious (Ibid.: 833)" model using this as the only variable in the model; this was argued to be biologically credible because humidity tends to be high only where rainfall and temperatures are high. The model results were then combined with a series of different GCMs to project the geographic areas that could be suitable for dengue transmission in the future, and population projections were used to calculate future populations at-risk. Under climate change scenarios, the authors estimated that 4.1 billion people could be living in dengue areas by 2055 and between 5.2 – 6 billion people by 2080.

Although this dengue model was, like the Rogers and Randolph (2000) model for malaria, based upon a statistical approach, the conclusions are very different: the Hales dengue model projects a significant expansion of risk, whereas the Rogers/Randolph malaria model does not. Clearly, dengue and malaria are very different diseases (not least of which being that malaria is a parasite and dengue is a virus) with very different transmission patterns. Yet as they are both mosquito-borne diseases, it is possible that some of the reasons behind the differences in the models relate to the assumptions embedded within.

In 4.3.4, the way in which socio-economic development was implicitly embedded in the Rogers/Randolph model was described. Intriguingly, Hales *et al.* selected data differently, and such that "the effect of human interventions is likely to be small (Hales et al., 2002: 833)". The difference lies in differing definitions of presence and absence points. Whereas the Rogers/Randolph model for malaria presence and absence points was based upon only the most recent malaria distribution data, the Hales model for dengue looked at the presence of dengue over a substantially longer time-frame (recorded outbreaks between 1961-1990). By using data from a longer time period, public health interventions such as vector control, which may be effective in the short-term, but hard to "sustain over time (Ibid.: 833)", would be more likely to be excluded from the analysis:

> **Hales:** ... given that there was a long period of time ... if any outbreak had occurred in that period we assumed that the climate in that region was suitable for an outbreak of dengue. We also assumed that if there hadn't been an outbreak reported that the climate was not suitable, which is probably not quite such a strong assumption to make, but in any case...

Thus, for example, if one region had recorded a dengue outbreak in 1973 but not since then, the Hales approach would record this as a "presence", but the Rogers/Randolph approach would record this as an "absence". The consequence is a looser definition of the environmental conditions suitable for dengue – the "environmental envelope" – in the Hales approach.

Another methodological factor that might influence the breadth of the environmental envelope is the number of variables used in the modelling. As mentioned, the dengue model eventually incorporated only humidity, although the authors effectively assume that this climatic variable accounts for both temperature and rainfall and they suggest that it did not affect model accuracy:

> **Hales:** ... actually for logistic reasons it was quite a bit of work at that stage to get the climate (variables) into the statistical model ... it was easier to make a model with only one variable. And it was indistinguishable, as I recall, in terms of how accurate it was.

This interpretation was strongly contested. A CCH opponent has argued that the more variables in the model, the narrower the environmental envelope:

> **R4:** The analogy is that there's no place like home for dengue or for any other disease. Now in terms of a single variable like temperature there are lots of places like home, there are lots of places with the same temperature where dengue exists at the moment that dengue doesn't actually occur. But if you're going to say, 'Well actually dengue needs temperature and rainfall of a certain specified amount', there are few places in the world that have the specified temperature and rainfall so your predictions of dengue's global distribution become more restricted. ... If you then try to predict ... dengue in a unidimensional world or dengue in a million-dimensional world, you can see in the unidimensional world temperatures are going to change very dramatically and therefore your predictions about dengue's future will change very dramatically. In a million-dimensional world... there will be no place in the future which is precisely like dengue's home at the present time.

The authors of the dengue model do in fact concede that their model accuracy was not perfect as it predicted contemporary dengue transmission in parts of Australia, for example, where vector-borne disease specialists were quick to point out their error (**Hales interview**). It would thus seem reasonable to conclude that the time period for the selection of disease presence and absence data, and the decision to model few or many variables can both affect the construction of an environmental envelope governing the transmission of a vector-borne disease.

Socioeconomic expectations revisited

In the absence of any scientific consensus on how to make such methodological decisions it is, once again, instructive to assess the way in which broader commitments have influenced research. One explanation relates to the different notions of "progress" in socio-economic circumstances held by the CCH opponents and proponents. CCH opponents, as has been alluded to earlier, tend to stress a more optimistic vision of the future, in which improvements in public health can be expected. Using the most recent data alone is most relevant for modelling precisely because it reflects, as accurately as possible, recent "progress" in disease control. CCH proponents, on the other hand, are rather more cautious (or pessimistic, depending on one's vantage point). Future social trends, Hales and co-authors note, “might move in a favourable direction (Hales et al., 2002: 833)”, but “there are other possible situations that might have negative consequences (Ibid.: 833)”. A concern with potential lapses in public health capacities is consistent with their vision of globalisation and its links to both climate change and infectious diseases (and which are furthermore consistent with the EID worldview):

> The resurgence of dengue fever and global climate change are driven by similar issues, which include excessive resource consumption in rich countries, an increase in social inequality, and population increases in poor countries (Ibid.: 833).

For their part, CCH opponents appear to be more optimistic about the future but less optimistic about human ability to assess it:

> **R4:** Very, very few models of health futures can take account of technological advances, of evolution of diseases, of changes in human susceptibility to disease, because we don’t know how much they will be and what their effects will be… Things that have revolutionised human history have been entirely unexpected. And 50 years down the line the future is very different from what it was expected to be...

This is consistent with their desire to re-frame the debate in terms of the present, emphasizing the need to control the disease in the present (an activity in which their expertise is rather more established). As Duane Gubler has said about climate change and its role in dengue re-emergence: “It’s all hype. A lot of public health officials and a lot of policy makers use global warming as a cop-out, an excuse for not controlling a disease that is very preventable (Phillips, 2008: A385)”.

4.6 Tick-borne disease and climate change: on the importance of place

The climate-VBD controversy has not been restricted to mosquito-borne diseases, as an extensive debate over the linkages between climate change and tick-borne diseases demonstrates. In this particular dispute, the importance of socio-economic variables in climate-disease modelling was brought to the forefront.

The dynamics of TBE transmission are complicated, leaving much room for interpretation. Its full transmission cycle involves the lifecycle of ticks, the climatic conditions that influence these lifecycles, the viral or bacterial infections that infect ticks and lead to disease, the natural animal "reservoirs" of ticks (such as deer populations), the land-use and ecological trends of these reservoir populations, and the factors that lead to or mitigate human exposures to ticks and risks of TBE. Amid this wealth of entry-points for research, CCH opponents and proponents focused on very different contexts and, predictably enough, they came to very different conclusions.

In the 1990s, there was an observed increase in the incidence of tick-borne encephalitis (TBE) in many parts of Europe, particularly central and northern Europe. Researchers began to investigate the reasons behind this, and the answers varied. Among the most common interpretations were detection bias (health authorities only started to properly monitor these diseases from the 1990s, so the increased incidence was only a consequence of increased awareness), social change following the collapse of the Soviet Union, and climate change.

In an article published in *Lancet* in 2001, two Swedish CCH researchers, Elisabet Lindgren and Rolf Gustafson, examined the increased incidence of TBE in relation to climate change (Lindgren and Gustafson, 2001). A central aspect of their hypothesis was that a "certain number of days per season (Ibid.: 16)" with a minimal temperature are required to sustain and increase the prevalence of tick populations. An important temperature threshold was 5°C, as it is required for tick activity and the onset of the vegetation season. The authors looked at historical records of TBE incidence in and around Stockholm county, Sweden, where records on TBE dated back to the 1950s. To conduct their analysis, the researchers looked at the two years of temperature data before a given year of TBE incidence (multiple years of weather are believed to impact tick prevalence in any given year) with data obtained from the Swedish Meteorological and Hydrological Institute.

Based upon multiple regression analyses, the authors concluded that TBE incidence was linked to a combination of climatic variables which, more generally, tend to include milder winters and earlier onsets of vegetative seasons. Noting that climate change had led to an earlier European vegetation season by an average of 12 days from 1960 to the mid-1990s and that winter temperatures had become milder, Lindgren and Gustafson conclude that a milder climate in Sweden "has contributed not only to increases in TBE incidences (Ibid.: 18)", but also to other tick-borne diseases such as Lyme borreliosis. It is noteworthy, however, that the authors also noted potential confounding variables in their analysis. Warm weather conditions, particularly in the spring and autumn, they suggest, also affects the human risk of getting tick bites as more people spend more time pursuing outdoor activities, whether mushroom picking, hiking, or picnicking. Additionally, a large number of outdoor cottages around Stockholm were built during the study's time-frame, thereby increasing the overall population at-risk. On the other hand, they noted, mitigating factors such as the availability of a vaccine against TBE and increased overall awareness about tick-borne diseases might have led to an underestimation of the strength of association between TBE and climate change.

The conclusions from this study were consistent with an earlier paper authored by Lindgren and colleagues in which she tracked a northerly expansion of the *Ixodes ricinus* tick species in Sweden and attributed it to climate change (Lindgren et al., 2000). It also corroborated with studies from the Czech

Republic, published shortly thereafter but already generally known at the time, which linked climate change to an expansion of *I. ricinus* ticks and TBE to higher altitudes (Daniel et al., 2003). These studies would form the basis for claims from the CCH community and beyond linking climate change to TBE, as a CCH opponent noted:

> **R17:** … the data that has most likely been referenced to demonstrate the linkages … between climate change and tick-borne disease have been of course the Lindgren work.

In similar fashion to the debates over malaria or dengue, these studies met immediate resistance.

In a letter to *Lancet*, Sarah Randolph accuses Lindgren and Gustafson of giving a "false impression of a simple causal relation (Randolph et al., 2001: 1731)" between TBE in Sweden and climate change, suggesting that although temperatures could be a limiting factor in TBE epidemiology, "the relation has not been satisfactorily shown (Ibid.: 1731)" by their analysis. Randolph also suggests that across Europe, the trends in TBE epidemiology were not uniform, leading her to conclude that "climate is not the sole causal factor (Ibid.: 1713)". Randolph concedes that climate change might be playing a role, but warns against inferring any pattern in the links between climate change in TBE. Vector-borne diseases such as TBE depend upon:

> a complex integration of biological processes, each of which responds differentially to changes in climate. Spatially variable climate change operating on a spatially variable climate base will throw up spatially variable changes in disease (Ibid.: 1713).

In other words, the variation is so substantial that it is not really worth attributing or predicting TBE incidence in relation to climate change. The key focus of Randolph's objections, however, relate to factors other than climate. TBE incidence rose dramatically in the 1990s in many European countries, including the Baltic States, Poland, Slovakia, the Czech Republic, and Germany, and Randolph suggests that “sociopolitical circumstances and resultant patterns of agricultural and leisure activities (Ibid.: 1731)” are the principal underlying factors. This would become the theme of much of her research. Primarily based upon studies of the Baltic States following the break-up of the Soviet Union, Randolph and her colleagues argue that agricultural reforms, a reduction in the use of pesticides, and increased unemployment and poverty are correlated with increased human exposures to ticks and to tick-borne diseases (Sumilo et al., 2007). In addition, a whole range of additional alternative explanations have been put forward by CCH opponents:

> **Randolph**: …the increase in tick abundance in central and eastern Europe can be related to the increase in deer abundance, which might be related to the change in hunting practise with socio-economic change.
>
> …But also, in the past, everything was blasted with insecticide and that no longer happens…
>
> …In 2006 we had this wonderful natural experiment in the whole across of Europe where there was a massive spike of TBE in some countries but not in all countries. And we showed this was to do with the weather. And it was … very good weather from June through to December 2006, perfect for mushroom growth and perfect for outdoor activities… And we showed that different countries showed different spikes in accordance with how you'd expect the local people to respond to good weather.

Addressing Randolph’s letter to *Lancet*, Lindgren and Gustafson justify the validity of the datasets they used for their analysis, hoping to prevent future “misinterpretations of the results (Randolph et al.: 1732)”. They claim that their study did take into account, indirectly, other “climate-dependent factors such as host animal availability and human leisure activities (Ibid.: 1732)”. The CCH community has furthermore defended its methodological approach:

> **R20:** In central Europe, TBE shows a year to year relationship to temperatures and so on and those issues will be a source of dispute because Sarah Randolph says ‘well that is just empirical epidemiology, you need to know what is going on with the three stages of the lifecycle of the tick or you don't know what you are talking about’. Well I think she is wrong, I mean epidemiologists often do operate empirically and for some questions that is entirely appropriate.

An irony here is that the CCH community is drawing upon “traditional” empirical epidemiological methodology to support this research, at the expense of “ecological” perspectives, even though as discussed in Chapter 3 they have argued that “traditional” epidemiology limits CCH research. Nonetheless, Randolph and her colleagues are not epidemiologists and this is a point that the CCH community has harnessed when on the offensive. Although Randolph identifies correlations between socio-economic factors and TBE in the Baltic States, the CCH community retorts:

> **R18:** … ultimately they stop short of coming to any causal inference because she ultimately only knows the associations. So, she's very much limited by her methodological approach and she has not been able to transcend the… confined area of ecology … if you do a proper epidemiologic study, you can control for all of the things that she is looking at ... I think a lot of that controversy can be resolved with a proper epidemiologic investigation.[22]

From their perspective, only the CCH community is equipped to conduct “proper” epidemiologic investigations, and where they have, they have sufficiently demonstrated the links between climate change and TBE.

On the importance of place

During an interview in which the climate-TBE controversy was discussed, one respondent discussed their opponent’s research:

> **R17**: This is a primary example of how long you didn't really listen at what question you were asking, but you use the answer to apply to a different question.

Both sides of the debate can probably be accused of this. The climate-TBE controversy is in fact one about whether a given question is the right one to ask, and about how far generalisations about the answers to these questions can be taken. Discounting for a moment the co-produced nature of climate-TBE

[22] This critique may have influenced Sarah Randolph. She is the senior author of a recent paper in which case-control study is used to examine socio-economic status and the risk of TBE in Poland (Stefanoff et al., 2012).

research, if one were to take the findings from both sides of the controversy at face value, the question would arise: should evidence from Sweden, where dramatic social upheavals have not generally happened, be offered as proof that climate change affects TBE in principal – or only in these regions in the specific time frames that the studies covered? Conversely, should evidence that social upheaval in Latvia or Lithuania led to a steep rise in TBE incidence be offered as evidence that social change, in principal, is the key determinant of TBE?

Of course, it is entirely possible that both sides are right, or that the truth is somewhere in between. But such has been the polarisation between CCH proponents and opponents that this middle-of-the-road position has barely been possible. Thus, the study sites adopted are ones most likely to confirm each side's original position. Actors appear to be aware of this:

> **R18:** I don't think anyone disagrees with the fact that climate is an important determinant of tick-borne encephalitis. The debate is over the degree. And Sarah Randolph has focused her research… in an area where there's been a big political upheaval that has precipitated a lot of social change, which is probably responsible for … a surge in a lot of different types of, of diseases, chronic and infectious. And TBE might be one of them.
>
> **R10:** I will say that it depends on which part of the world you are looking or which part of Europe.

The definition of place thus becomes an integral, if slightly unspecified, component of the debate. A CCH opponent discussed the research from Sweden predicting a northerly expansion of TBE:

> **R17:** What proportion of the European land surface does that concern? And that's rather small. And how many people live there or indeed walk through there as visitors or as tourists? … Can that in any way explain the massive increase in incidence in Europe? And the answer is: not possible. You might get one or two cases, but hang on, we're talking about an extra 2000 cases, where do they come from? They must have come from somewhere else, from a different cause.

Importantly, this comment does not refute the possibility that climate change is responsible for a northerly expansion of TBE in Europe. Indeed, CCH opponents even concede this point! Sarah Randolph and David Rogers, for example, have even published a model predicting an expansion of TBE in northwest Sweden (and a contraction of TBE at the southern edges of its European distribution range) (Randolph and Rogers, 2000). Consider also that Lindgren and Gustafson do not claim to be demonstrating a climate-TBE link for all of *Europe* (their *Lancet* paper is even titled "Tick-borne encephalitis in Sweden and climate change"), but in the quote above *Europe* is used as evidence to weaken the climate-TBE link. The converse also happens, of course. Sweden and/or Western Europe are used to disprove, or at least limit, the impact of findings from the Former Soviet Union. What is behind this distortion of the terms of debate? A plausible conclusion is that the stakes surrounding the climate-TBE controversy related to the *generalisability* and *uptake* of the findings: which studies will be more widely cited among scientific and policy communities? Actors on both sides fear the other's studies will prove definitive. In this way, conflicting professional interests have, once again, resulted in different interpretations of the importance of the threat posed by climate change. For CCH opponents it is insignificant in comparison with more proximate factors, which, incidentally, are easier to address:

> **R17:** The thing is we are looking at the wrong target or we are looking at the wrong cause. Even if climate change is playing a part, the evidence is that it's playing a rather small part. And what could we do about it? We could go home, beat our chests and turn off the light bulb...

For CCH proponents, it is a call to arms: "We need to adapt, because … the climate is going to continue to change regardless of the actions we are taking from mitigation right now (**R10**)".

Thus divided, both sides continue to produce papers emphasizing or re-iterating their original stance, supported by new data, new models, and new collaborations (e.g. Jaenson and Lindgren, 2011, Randolph, 2010, Stefanoff et al., 2012).

4.7 Discussion: controversy and uncertainty

In this Chapter, a long-standing and heated controversy over whether, and to what extent, climate change can be said to influence the transmission of vector-borne diseases has been examined. The focus of this Chapter has been to analyse CCH research in detail so as to unearth the deeper reasons behind the climate-VBD controversy. After introducing the general terms of the debate (4.1), it was noted how the boundary work evident in the controversy offers a useful, if ultimately incomplete, insight into the nature of the debate (4.2). Four case studies were subsequently presented in order to further explore the controversy: malaria (4.3), "highland" malaria (4.4), dengue (4.5), and tick-borne diseases (4.6). In this section, some of the key themes emerging from these case studies will be summarised.

The climate-VBD controversy has been described as one between CCH proponents and CCH opponents, the latter primarily consisting of a constellation of ecologists and "infectious disease specialists" (4.2). This is admittedly an over-simplified way of classifying these actors. Despite that CCH proponents and opponents share very similar worldviews and research objectives, it should not be assumed that these "sides" are so harmonious that they agree with each other on every point. Even the tight-knit constellation of CCH proponents (Chapter 3) has its internal rivalries and jealousies, and some degree of boundary work can be seen among them. Paul Epstein, for example, was denounced by one CCH community member as being "only" a medical doctor and not an epidemiologist; his work was described as overly simple and polemic (private correspondence). Similar rivalries exist amongst the CCH opponents. Nonetheless, as evidenced by the arguments that key CCH opponents and proponents have consistently supported, such a classification appears to be both fair and valid.

The publication of the Martens malaria models was a landmark event for CCH research. It was also the tipping point in the CCH controversy. Before the publication of these models, a group of ecologists and infectious disease "specialists" may have been annoyed by CCH community research, but it was not perceived to be much of a threat – as pointed out in Chapter 3, CCH research was originally marginal even within the public health and epidemiologic worlds. Everything changed with the attention that the Martens models received. "The die was cast and persists today", as Sarah Randolph (2010: 93) describes it. The media became interested, the IPCC republished the maps, and the ecologists became so frustrated that they mounted a counter-attack. The spats became unpleasant and an arms race of high-profile publications ensued.

The controversy between CCH proponents and opponents was not, of course, restricted to the peer-reviewed literature. Shouting has broken out at conferences. Disagreements have extended into "official" fora, such as IPCC review panels (**Chapter 5**). It has been covered by the print media, and aspects of it have even appeared in films. Al Gore's film *An Inconvenient Truth* discusses Paul Epstein's account of highland malaria, which drew the ire of Paul Reiter and in turn motivated him to appear in the film *The Great Global Warming Swindle*:

> **Reiter:** It's nauseating. Gore shows a little animation of mosquitoes moving up a mountain…There were actually six people given special credit at the end of his film (An Inconvenient Truth), and one of them is Paul Epstein.[23]

All of this to re-iterate that CCH research outputs are visible and contested beyond the scientific world: the stakes are higher than scientific prestige alone. To understand the roots of the controversy, it has been instructive to examine the co-production of CCH research. The case studies presented in this chapter

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[23] From the transcript of an interview with Paul Reiter, available at: www.21stcenturysciencetech.com/Articles%202007/GW_malaria.pdf (accessed October 17, 2011).

(4.3-4.6) allude to a few formative ways in which differing disciplinary interests have driven the climate-VBD controversy.

4.7.1 Modelled worldviews

Assessing the climate-VBD controversy through a theoretical perspective informed by the idiom of co-production (Chapter 2) offers a much greater explanatory power than what has been achieved by other analyses of this controversy, such as the "collective action frames" approach discussed in 4.2. By focusing attention to the ways in which the political and the scientific have mutually informed this controversy, it can be seen that seemingly routine methodological issues – such as whether to model future disease outcomes via a systems or statistical approach (4.3), how to define disease presence/absence points (4.5), or where to situate research (4.6) – are in fact as much the product and consequence of subscription to particular worldviews and policy preferences as they are a product of technical decision-making.

As Randolph and Rogers have recently noted: "The type of model, the variables used, and the way they are selected are important determinants of the final model fit (Randolph and Rogers, 2010)". Yet, as this Chapter has shown, regardless of which disease research is focused upon, and regardless of which modelling methodology is used, the single-most important determinant of the final model fit appears to be the person who undertook the research.

Although it is fairly certain that both CCH opponents and proponents would like for VBDs to become less of a public health burden, it is clear that the two sides vehemently disagree about the best way of doing so. It is precisely because the future is unsettled and because no dominant disease control regime for addressing vector-borne diseases currently exists that the controversy is so intense: "winning" the controversy would mean not only scientific recognition but also the opportunity to influence the allocation and direction of limited global resources dedicated to disease control. This dynamic influences both the content and the context of the controversy.

Differing worldviews and policy preferences are, in turn, closely related to the disciplinary context within which the CCH proponents and opponents operate, as Chapter 3 demonstrated for the CCH community. Discipline, for example, influences the manner in which they approach the research and interpret its findings:

> **R1:** We're relatively comfortable to try and make a judgement about what would happen to that disease system if some of the major forcing factors were changed. Epidemiologists are happy to do that. Whereas I think that makes the entomologists and the outbreak investigators quite uncomfortable because they feel it's just so much that's uncontrolled and uncertain that really you've got no right to draw any conclusions at all because of all the factors that are involved.

Similarly, disciplinary perspectives appear to be influencing whether the future is viewed optimistically or pessimistically. This in turn shapes the way in which a researcher views the past, present and future of VBD spread:

> **R1**: I suspect that another dynamic underlying this difference of opinion is the extent to which you believe humans control nature and will be able to hold that control in the future.

As the case studies surrounding malaria (4.3.3) and dengue (4.5) demonstrate, CCH opponents and proponents hold very different perspectives on how the future might play out, and these perspectives are implicitly incorporated into their models. It is impossible to say whether this is because a particular vision of the future supports their preferred research and policy conclusions, or vice-versa, but most likely there is an iterative relationship between the two.

4.7.2 Assumptions and the evidential context: factors preventing "closure"

In the climate-VBD controversy, for all the reasons specified above, the "ecologists" or "infectious disease specialists" resist their potential marginalisation in the field of vector-borne diseases by what they consider to be a group of amateur interlopers. Accordingly they denounce the credibility of these interlopers as well as the strength of the associations between VBDs and climate change. The CCH community, meanwhile, attempts to do the opposite with the aim to solidify an emerging field and gain influence with key stakeholders.

As this power struggle is so highly polarised, it is plausible that the areas of disagreement have been over-emphasised while the areas of agreement have been under-emphasised. It is noteworthy, for example, that nobody disputes claims such as that climatic factors influence the habitats and survivability of ticks or mosquitoes. Additionally, in 6.6 it was noted that CCH opponents and proponents even agree that TBE is likely to expand in northern Sweden under climate change scenarios. It is even possible that a more substantial consensus exists than a reading of the literature would suggest:

> **R1:** And what does lie behind it (the climate-VBD controversy)? I think it's a different view of the world. I mean, I think if you look closely at the science, there isn't actually any disagreement about the essential points in the science. It's more a framing issue.

It is certainly possible that at stake in this controversy is the uptake of CCH research – the way in which the results might influence priority setting in the public health world. This could help to explain why actors on both sides are fighting not only over the "truth" of claims, but also over the implications and the generalisability of these claims. One ramification of this could be that actors on both sides of the controversy have attempted to misrepresent the claims made by their opponents. For example:

> **R6:** It's interesting - that straw man that's going to put up. It's been put up by folks who don't want us to talk about climate change. In fact if you go back and look at the publications on climate change and health ... it doesn't reflect the reality of where the research is and what the writings say.

One way of viewing this is through the language of Pinch (1985): it is the "externality" of the research claims that is contested here. Actors on both sides hope to establish as broad an "externality" for their claims as possible while still remaining credible. There is a delicate balance between the two:

> Reports of high externality will stand a greater chance of making a contribution to the wider corpus of knowledge in view of their high evidential specificity, but such reports will also be risky ... because they involve so many aspects of the observational situation, and thus give more grounds for possible criticisms (Pinch, 1985).

It is not very interesting, but much safer, to argue that the rainfall can influence malaria, or that one mild winter in Sweden was favourable for tick populations and that this is part of the explanation for a higher TBE incidence. It is something entirely different to argue that what the same data actually says is that climate change caused the mild winter and is likely to cause more mild winters and will likely drive the risk of TBE across not only Sweden but also Europe in the future. Similarly, everyone can agree that in a million-dimension world, there is "no place like home" for dengue or any other disease (4.4). But to say so would be hardly publishable, and even if it were it would certainly not have any impact in scientific or public health circles.

According to this reading, there are two dimensions of the climate-VBD controversy which make it particularly resistant to "closure". One relates to the breadth and newness of the topic – its "evidential context" is not well defined. A second relates to the plurality of arenas in which claims to externality are made. These points will be further discussed below.

4.7.3 Multiple assumptions and the uncertainty cascade

The production of and controversy surrounding research linking climate change with malaria (4.3), "highland" malaria (4.4), dengue (4.5) and tick-borne disease (4.6), depends upon an unusually wide range of assumptions, some implicit and some explicit. These, as has been documented, include assumptions about everything from the validity of methodology of specific modelling techniques, the validity of climate data and of GCM outputs, the plausibility of socio-economic futures, the accuracy and relevance of a wide range of biological parameters, and so on.

This predicament is quite unlike "normal" science, in which "background assumptions are routinely accepted (Pinch, 1985: 14)". Instead, all of the research can be easily contested: "Because the results are only as good as the assumptions used to get them, to doubt the assumptions is, in effect, to doubt the results (Ibid.: 14)". With so many assumptions that can be called into question in CCH research, there is much scope for controversy.

This scope is further broadened by its reliance on key inputs from other disciplines. The concept of a "cascade of uncertainty" or "uncertainty explosion", discussed in 4.4, is well-known within the climate change community. As the IPCC describes it, it is "the process whereby uncertainty accumulates throughout the process of climate change prediction and impact assessment"[24] (see also **Figure 3**). This range of uncertainties can cast a wide range of doubts upon any findings from climate impact research. As the debate over highland" malaria demonstrated (4.4), it can also lead to a controversy over data inputs that are quite outside the core competency of the climate impact researchers. This creates a dependency on "upstream" modellers, such as those in the GCM modelling community. To work with climate data, researchers must, to various degrees accept at face-value the climate models they receive from the GCM modelling community. In practice, it is likely that the CCH modellers are aware of the many uncertainties inherent in GCMs, but to push forward with their own research they must, to a considerable degree, suppress whatever uncertainties they might have.[25] More generally, the practice

[24] http://www.ipcc.ch/ipccreports/tar/wg2/index.php?idp=108#fig22, accessed October 20, 2011.

[25] This is consistent with Donald MacKenzie's notion of the 'certainty cascade' (MacKenzie, 1990), in which he notes that the users rather than producers of models tend to have higher certainty about the models. What we observe here are "users" that are also "producers" (of

of using model outputs as inputs to other models only serves to amplify uncertainties, thereby creating the possibility for ever more technical debate. Where this debate turns to the model inputs (GCMs in this case), it is further removed from the core expertise of the key actors in the controversy.

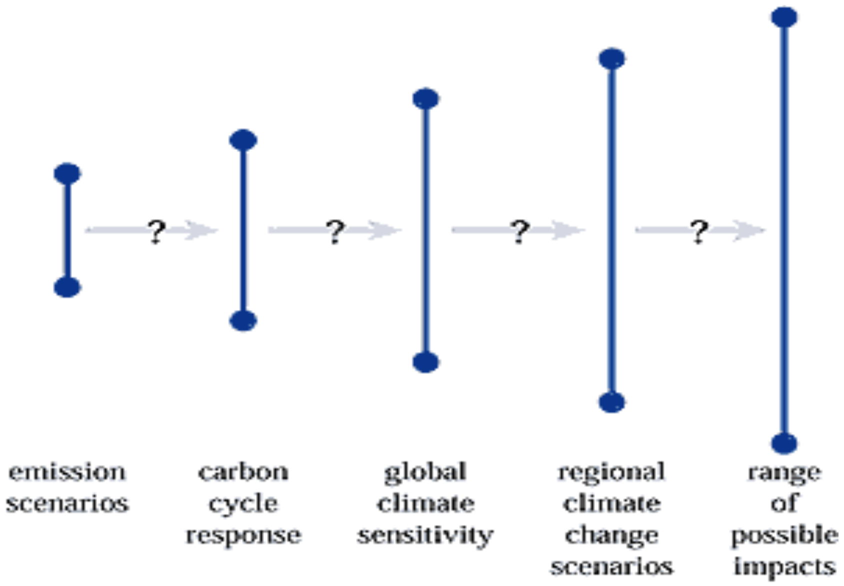

Figure 3. The uncertainty cascade

The range of major uncertainties typical in climate change impact assessments, showing an "uncertainty explosion" as these ranges are multiplied to encompass a comprehensive range of future consequences, including physical, economic, social, and political impacts and policy responses.[26]

It is only to be expected that a controversy over climate-VBDs should eventually focus upon the realities of climate change itself. Again, Pinch's work on externality offers an especially useful insight:

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climate-disease models), who out of necessity have a fairly high certainty of the models that they use as data inputs.

[26] Image and sub-heading from IPCC. The term cascade of uncertainty comes from the late Stephen Schneider, and the term uncertainty explosion comes from Henderson-Sellers (1993). Accessed from: http://www.ipcc.ch/ipccreports/tar/wg2/index.php?idp=108#fig22, October 20, 2011.

> In general, the lower the degree of the externality of the report challenged, the more damage will be done to the experiment. This is because a successful challenge, in effect, makes redundant all of the observational process...at higher levels in the chain of inference (Pinch, 1985: 25).

Any attack, properly sustained, on the validity of climate or climate change data – or any of the other key inputs into climate-VBD models – is a strong blow to the overall conclusions of the model. This helps to explain the heated debate about climate data in the example of highland malaria (4.4) or why Sarah Randolph re-assessed the climate data used in Lindgren and Gustafson's research on climate and tick-borne diseases (4.6).

4.7.4 The future is not a testable hypothesis

A second factor contributing to the sustained nature of the climate-VBD controversy is its undefined evidential context. Even excluding the wide range of assumptions involved in modelling the links between climate change and the spread of vector-borne diseases, the "evidential context (Pinch 1985, 10)" of this research is incredibly "loose". It is a relatively new, largely future-orientated research area dependent on another highly uncertain and contested field, climate change research, for some of its primary inputs. Additionally, the research is focused on identifying the importance of one factor influencing highly multi-factorial systems – the transmission cycles of vector-borne diseases are the result of a complex interplay between pathogens, vectors, reservoir hosts and dead-end hosts, each of which in turn is dependent upon and influenced by a huge range of environmental, biological, social, and economic forces. Proving or disproving a link between climate change and a VBD is, scientifically, nearly impossible:

> **R18:** The problem is if you then overlay these climatic models ... there are a lot of... economic, developmental issues that feed into these models, like the IPCC models, they all depend on different emissions scenarios. And the emissions scenarios aren't predetermined because we don't know where we as a society are going. Are we going to reduce our CO_2...? And so, because of all the societal unknowns, the models range quite a bit. And once you overlay this range of

> models over your current [disease] models ... then you will lose that signal. So you will not be able to identify an association.

In practice, of course, "proof" only exists if a group of scientists believes that it does. Such acceptance necessarily depends upon some agreed-upon standard of proof. Yet, as has been shown through each of the case studies in this Chapter, there are no agreed-upon standards of proof in CCH research. This problem is even more acute where this research involves modelling the future (and the extent to which this is considered to be a valid scientific enterprise varies). This quandary is an example, *par excellence*, of the experimenters' regress. In his famous case study on the controversy surrounding the detection of gravity waves, Harry Collins describes it thus:

> What the correct outcome is depends upon whether or not there are gravity waves hitting the Earth in detectable fluxes. To find this out we must build a good gravity wave detector and have a look. But we won't know if we have built a good detector until we have tried it and obtained the correct outcome! But we don't know what the correct outcome is until and so on ad infinitum (Collins 1985: 84).

Without time travel, it is not possible to test whether or not future climate-VBD models are good. There is no possible way for this circle to be broken. As such, the experimenters' regress "can only be avoided by finding some other means of defining the quality of an experiment; a criterion must be found which is independent of the experiment itself (Ibid*.:* 84)". In some cases, this might be the demonstration of some proxy measure of reality, but often the acceptance of truth-claims in these situations boils down to whether or not a scientist and her methodologies can be trusted or not. This explains the highly personal nature of the boundary work involved in the climate-VBD controversy and sheds light on why some have said, as we have seen, that the best way to judge a model of the future is to judge the track-record of the scientist in assessing the present and the past. The credibility stakes in this controversy are so high precisely because the evidential context is so loose.

4.7.5 Multiple venues for truth claims

In a highly polarised controversy that is particularly difficult to resolve, scientists might resign themselves to the possibility that "closure" might not be reached anytime soon, perhaps not even during their career. Yet, if they work in a research area (like CCH) that has practical policy and public health implications, they might attempt to orchestrate closure in other venues, thereby circumventing their main opponents.

An important part of the context shaping the climate-VBD controversy is that there have been, throughout its history, calls from global stakeholders – WHO, national governments, etc. – for answers. The highland areas of Africa, or indeed dengue-vulnerable regions of the Western world, cannot wait until a longstanding controversy is resolved before acting. They want to know if they face a risk and, if so, what they can do about it: public health is largely predicated on pro-active preparedness. The policy context has influenced the climate-VBD controversy on at least three different levels! First, the policy context has created an impetus for this research, and also provided funding for it. Second, possible policy responses are incorporated into climate and climate-disease models. Third, the policy world has intervened to attempt to resolve the issue. This has brought the controversy to new arenas, and notably to IPCC where, through publishing Assessment Reports, there has been a strong attempt to forge consensus in this highly polarised field. The manner in which this has happened, and the ramifications for the climate-VBD controversy as well as the prioritisation of approaches for controlling vector-borne diseases, will be the focus of Chapter 5.

4.7.6 On controversies and opportunism

Controversies, such as the one described in this Chapter, are certainly great study material: the manner in which extra-scientific factors are mobilised in order to support particular positions offers fascinating insights into the co-production of knowledge alongside political and policy spheres. Yet the implication is often that controversies ultimately end up having their winners and their losers – only one side of the debate can ultimately prevail. This may be true in the grand scheme of things, but it could also be the case that actors on both sides of a controversy are well aware that the attention that controversies generate can be beneficial to all involved.

It is worth noting, for example, that the substantial volume of peer-reviewed outputs produced in the climate-VBD controversy might not have been possible if the controversy did not exist. High-profile journals may not have found the papers, letter and commentaries to be as interesting in the absence of a controversy, meaning that the visibility – and thus the profile – of the topic as a whole, as well as of individual researchers, would be lesser. Given the interrelationships between publishing and funding, this is no minor point. Participants in the CCH controversy, from both sides, are quite aware of this:

> **R18:** A lot of the climate sceptic people, they have been funded under all this climate research as well, and so they're quite happy to take the money despite the fact that they don't subscribe to... climate change as a scientific fact or as a phenomenon.

> **R13:** Obviously, the whole climate change debate has nurtured the tremendous amount of grant giving, which is something that I feel comfortable about because ... basic research ... is very important.

The growth of funding for research exploring climatic and environmental links to vector-borne diseases has been fairly substantial, particularly in Europe, as will be discussed in Chapter 5. CCH proponents and opponents in some instances even find themselves in the same EU-funded projects, and they have even been known to publish together. Aside from demonstrating a certain degree of opportunism, this explanation demonstrates that funding is not one of the key components at stake in the climate-VBD controversy. Quite the opposite – the controversy has stoked funding and this has benefited both sides.

Ideologically, it is important to note that although the climate-VBD controversy has been rather heated and even deeply personal in some instances, actors on both sides of the controversy ultimately claim that it has been good for science:

> **R4:** And because the models are formulated in quite different ways, they have quite different assumptions, quite different variables, they're likely to come to different conclusions. This is entirely healthy. This is how science makes advances. This is the really important for founding our understanding of the way the world works.

> **R19:** Okay I think the issue of attribution…It's a very difficult theoretical and methodological question and will remain so, okay? And there will be always controversies and I think controversies are actually very healthy.

It is striking that a longstanding controversy has not managed to shake the contestants' convictions that their work is anything but "scientific". It is tempting to view such commentary as no more than another reminder of the Janus-faced nature of science, deeply political and controversial internally but statesmanlike and objective externally (Latour, 1987). Yet there is another possible explanation: the scientists themselves are so deeply entrenched in their work that they, too, cannot separate the science from the politics.

Chapter 5. Science and Politics at the IPCC

"When the prices to pay are so large, the knowledge on which these kinds of decisions are taken had better be right. The science must be irreproachable".[1]

5.1 Introduction: Climategate and the IPCC

The 2007 Nobel Peace Prize was famously shared between Al Gore and the Intergovernmental Panel on Climate Change (IPCC) for "their efforts to build up and disseminate greater knowledge about man-made climate change, and to lay the foundations for the measures that are needed to counteract such change".[2] At that time, the IPCC was enjoying "a pristine reputation (Jasanoff, 2010: 695)" and the public acceptance of climate change science may have never been higher. Yet just two years later, towards the end of 2009 and coinciding with UNFCCC COP15 meeting in Copenhagen, the IPCC would find itself in the middle of a highly visible and contentious scandal.

"Climategate", a "big embarrassment to the IPCC (**R22**)", arose when thousands of e-mails and documents of prominent scientists at the Climate Research Unit (CRU) of the University of East Anglia, one of the leading producers of global climate datasets and models, were hacked and subsequently publicised on the internet (Science and Technology Committee, 2010). The hacked files offered a rare behind-the-scenes glimpse into the machinations of climate science. In one highly publicised leaked email, Phil Jones, head of CRU, vowed to exclude a few papers from the IPCC assessment report[3]:

> I can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow - even if we have to redefine what the peer-review literature is! (Cited in Monbiot, 2009)

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[1] Science and Technology Committee, House of Commons (UK) (2010).
[2] http://www.nobelprize.org/nobel_prizes/peace/laureates/2007/, accessed November 28, 2011.
[3] Both papers did in fact make it into the IPCC 4th Assessment Review. See: Heffernan (2010).

In another, scientists discuss the tricks-of-the-trade in replacing tree-ring proxy temperatures with recorded temperatures for recent years when constructing long time-series temperature trends:

> I've just completed Mike's Nature trick of adding in the real temps to each series for the last 20 years and from 1961 for Keith's to hide the decline (Cited in Hickman and Randerson, 2009).

Unsurprisingly, these emails were widely cited by prominent climate change sceptics as evidence of foul play in climate research. As Myron Ebell, director of energy and global warming policy at the Competitive Enterprise Institute, told the *Washington Post*: "Some of the e-mails that I have read are blatant displays of personal pettiness, unethical conniving, and twisting the science to support their political position (Cited in Eilperin, 2009)".[4] The climate scientists went on the defensive: "They are trying to pick out minor things in the data and blow them out of all proportion (Heffernan, 2010: 26)", as Phil Jones retorted.

In addition to *Climategate*, making matters worse for global climate change science, reports surfaced in early 2010 about various errors found in the IPCC Fourth Assessment Report (AR4). Most famous, dubbed by some "Himalayagate", was that IPCC misstated the total land covered by Himalayan glaciers and misquoted a paper predicting the year by which the glaciers could disappear, citing 2035 instead of 2350 (Pearce, 2010). As the source of this data turned out not to be from the peer-reviewed literature, IPCC peer review processes were criticised and its Chair, Rajendra Pachauri, faced pressure to resign (Bagla, 2010).

Although it seems clear that *Climategate* was timed to coincide with and disrupt COP15, it would be very challenging indeed to assess the extent of the impact of *Climategate* on the UNFCCC process as narrowly defined. Among the most immediate impacts was that as climate scientists and policy-makers gathered in

[4]http://www.washingtonpost.com/wp-dyn/content/article/2009/11/20/AR2009112004093.html

Copenhagen for the UNFCCC COP15 negotiations, they were forced to address an onslaught of media questions about the hacked files, in some instances distracting them from the negotiations.[5] Nonetheless, the Copenhagen Accord, (which recognised the desire to limit global average temperature increases to 2°C and required nations to pledge emissions cuts[6]) was eventually agreed upon by 114 State Parties at COP15[7] (although, as is nearly inevitable in the negotiation of any international treaty, to varying levels of enthusiasm (Bodansky, 2010)).[8] *Climategate* may also have influenced public attitudes towards climate change. As most State Parties involved in UNFCCC negotiations are democratically elected governments, public opinion plays an important, if indirect, role in motivating governments to make binding commitments to greenhouse gas emissions reductions. Numerous polls demonstrated that public belief in climate change had noticeably declined in the wake of *Climategate* and, intriguingly, these findings have not been restricted to the general public. One study suggested it even undermined belief in global warming among American TV meteorologists (Maibach et al., 2011).[9]

Collectively, *Climategate* and *Himalayagate* greatly challenged the credibility of climate change scientists and science and, by association, some of the principal institutions producing and vetting climate change knowledge, namely CRU and IPCC (e.g. Jasanoff, 2010; Monbiot, 2009; Revkin, 2009). IPCC in particular faced

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[5] Numerous interviewees made this point.
[6] http://unfccc.int/resource/docs/2009/cop15/eng/11a01.pdf, accessed December 1, 2011.
[7] http://unfccc.int/meetings/copenhagen_dec_2009/items/5262.php, accessed November 28, 2011.
[8] It has also been suggested that despite the Copenhagen Accord, "no agreements of any consequence" had been reached at COP15 (Prins et al., 2010).
[9] Public opinion polls of climate change have shown a decrease in the public acceptance of climate change post-Climategate. For example, a poll conducted in February, 2010, demonstrated a 30% drop in the number of British adults who believed that climate change is "definitely" real (Jowit, 2010). Similarly, a 2011 survey of 25,000 internet users from 51 countries revealed that although more people were concerned about climate change in 2011 than in 2009, the total percentage (69%) was still lower than the 2007 percentage (72%) (Chestney, 2011). Nonetheless, the results from a US poll conducted in autumn, 2011, demonstrates both a decline in public belief in climate change following *Climategate* as well as a modest rebound in this belief. The pollsters note that public belief is highly influenced by personal weather observations. See: http://www.brookings.edu/~/media/Files/rc/papers/2012/02_climate_change_rabe_borick/02_climate_change_rabe_borick.pdf, accessed March 3, 2012.

numerous criticisms related to its potential bias and flawed peer review procedures, harming the perceived reliability of its assessments and necessitating organisations such as the InterAcademy Council (IAC) to review IPCC (IAC, 2010, Berkhout, 2010). Meanwhile, the UK House of Commons Science and Technology Committee was mandated to investigate *Climategate*. It ultimately exonerated Phil Jones from any wrongdoing on the grounds that his actions were "common practice" within the scientific community (Science and Technology Committee, 2010) – even though these practices were less than ideal and included "routine refusals to share raw data and computer codes (Jasanoff, 2010: 695)". As the Committee noted:

> Reputation does not … rest solely on the quality of work as it should. It also depends on perception. It is self-evident that the disclosure of CRU e-mails has damaged the reputation of UK climate science… If the practices of CRU are found to be in line with the rest of climate science, the question would arise whether climate science methods of operation need to change. In this event we would recommend that the scientific community should consider changing those practices to ensure greater transparency (Science and Technology Committee, 2010).

This finding is both ironic and significant. It implies that rather than illuminating anything particularly unusual about scientific practice to the world, *Climategate* simply offered a rare insight into the type of work consistent with everyday scientific practice – and that was enough to cause such furore.

Jasanoff has recently argued that contemporary science needs to meet public expectations "not only about its products but also about its processes and purposes (Jasanoff, 2010: 696)". The fallout from *Climategate* reveals just how serious the implications can be when science fails to meet such expectations. Although *Climategate* is certainly not the first instance in which the credibility of IPCC has been challenged[10], it does serve to demonstrate just how precarious

[10] In 1996, to give one early example, the IPCC 2nd Assessment Report and its peer review processes came under intense scrutiny after an eminent physicist, Frederick Seitz, famously published a letter in the *Wall Street Journal* claiming "I have never witnessed a more disturbing corruption of the peer-review process than the events that led to this IPCC Report" (Seitz, 1996).

and tenuous the credibility of climate science continues to be. There consequently continues to be a need to better understand the processes through which IPCC assesses and validates scientific knowledge: how it is "warranted", in the language of Shapin and Schaffer (1985). A role for interpretative accounts of the IPCC has been previously identified by numerous authors (Beck, 2012, Hulme and Mahony, 2010, Shackley and Skodvin, 1995, Yearley, 2009, Hulme, 2008), who have emphasised, among other topics: the need for critical analyses of how IPCC is governed; analyses of how IPCC attempts to establish credibility; critique of the implications of the IPCC peer review process; and insight into how knowledge claims (uncertain, controversial and situated) find their way into IPCC reports.

In Chapter 3, the community involved in the production of CCH research was described in detail. Their motivations were described, as were the ways in which they managed to obtain key positions as lead authors and reviewers in the IPCC assessment process. In Chapter 4, the ways in which normative and political preferences are deeply embedded in the contents of CCH research were revealed through an analysis of the longstanding climate-VBD controversy. It was noted that the debate, highly polarised, is exemplary of the experimenters' regress and is not one that can be resolved purely on scientific grounds alone. Drawing upon both of these Chapters, this Chapter will seek to provide a detailed case study of the "warranting" procedures of IPCC by focusing on the IPCC treatment of CCH and, in particular, the transposition of the climate-VBD debate from the peer-reviewed literature to the IPCC arena.

This Chapter consists of four sections. In 5.2, the history, organisation and mandate of IPCC will be described, providing the necessary context about how and why IPCC goes about assessing climate science. Thereafter, 5.3 will examine how the health chapters have been produced, paying particular attention to the way in which the climate-VBD debate was interpreted and communicated by the IPCC 3rd and 4th Assessment Reviews.

5.2 The IPCC: history, structure, processes

5.2.1 A brief pre-history of the IPCC

Following and building upon the discussion from 1.4 about the "global" framing of climate change, this section will focus on the key international events that led to the establishment of the IPCC. A brief review of this period – which is much more extensively assessed and recollected elsewhere (Agrawala, 1998a, 1998b, 1999, Boehmer-Christiansen, 1994a, 1994b, 1994c, Hecht and Tirpak, 1995, Bolin, 2007) – demonstrates that IPCC is both an agent and result of co-production, as does a description of the IPCC mandate (5.2.2) and processes (5.2.3). All of this is essential background for an analysis of how IPCC has treated CCH science (5.3).

The activities of the World Meteorological Organization (WMO), established in 1950, have been particularly instrumental to the IPCC's history.[11] One of its two "parent" agencies, WMO actively forged international research networks and standards in the field of meteorology and climatology, enabling global-scale understandings of atmospheric dynamics and playing a key role in laying the foundations upon which climate change would eventually be understood (Miller, 2001, Edwards, 2010).[12]

Concurrently, in response to the wide-ranging environmental concerns that began to gain prominence during the 1960s (e.g. Jasanoff, 2001), the UN organised the 1972 Stockholm Conference on the Human Environment, the first UN meeting which specifically addressed humans and their impacts on the environment. The Stockholm Conference led to the establishment of the United Nations Environment Programme (UNEP) and recommended that the World Meteorological Organisation (WMO) should continue with its Global

[11] WMO originated from the International Meteorological Organization, which was founded in 1873. WMO became operational in 1951. See: http://www.unctad.info/en/World-Statistics-Day-Geneva/Events/World-Meteorologicla-Organisation-WMO/, accessed December 29, 2011.
[12] The other parent agency of IPCC is the United Nations Environment Programme.

Atmospheric Research Programme (GARP)[13] and, if necessary, establish new programmes to better understand climate change (Bolin, 2007). The subsequent Stockholm Declaration anticipated the need for organisations like IPCC.[14] Principle 20, for example, states: "the free flow of up-to-date scientific information and transfer of experience must be supported and assisted, to facilitate the solution of environmental problems".[15]

In the following years, awareness of the possible links between carbon dioxide emissions and global warming would grow, in part through the profile given the topic by a GARP conference focused on climate modelling. This meeting, which took place in Stockholm in 1974, was organised by the eminent Swedish scientist Bert Bolin, who would later become the first Chairman of IPCC (Bolin, 2007). At this meeting, the results from general circulation models predicting an average warming of 3°C, given a doubling of CO_2 concentration in the atmosphere were presented as were a range of other talks about the various potential impacts of climate change (Ibid.). Agrawala notes that, in combination with some of Bolin's own research on the contribution of land degradation to atmospheric CO_2, the research discussed at the GARP conference "provoked serious discussion as to whether human induced climate change was indeed occurring and if it might seriously impact global society (Agrawala, 1999: 160)".

Much of this discussion would continue at the international level. The 1975 WMO World Congress debated climate change and concluded that although there was no imminent danger, there was enough reason to be concerned and to

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[13] GARP was a collaboration between WMO, a UN agency, and the non-governmental International Council of Scientific Unions (ICSU). This collaboration was the outcome of a speech to the UN by US President Kennedy in 1961 in which he called for peaceful uses of outer space, notably global efforts at weather research and prediction. See Argawala (1999).

[14] It is worth noting that one of the authors of the final report on the Stockholm Conference was the microbiologist and promoter of ecological thinking in health, René Dubos (see 3.2) (Ward & Dubos, 1983).

[15] http://www.unep.org/Documents.Multilingual/Default.asp?documentid=97&articleid=1503, accessed December 27, 2011. This sentiment was echoed in the report on the 1972 Conference in which one section was dedicated to climate change and in this section the need for a new capacity for global decision-making is stressed.

promote further research (Boehmer-Christiansen, 1994a). In 1975, the UN General Assembly requested WMO to study climate change, and it responded by setting up an early climate change panel of experts (Ibid.). Meanwhile, climate change broadened as an issue with the entry of geologists and ecologists through the influential International Council of Scientific Unions' (ICSU) Scientific Committee on Problems of the Environment (SCOPE). In 1976 this committee reported that rapid increases of atmospheric CO_2 made ever-more urgent the need for additional research (Ibid.). In 1978, GARP organised a second climate conference, which planned further climate research but did not specifically address anthropogenic climate change (Bolin, 2007). Rather more significant, however, was the first World Climate Conference, organised by WMO and hosted in Geneva in 1979,[16] which highlighted the scientific uncertainties surrounding climate change, led to the establishment of the World Climate Programme (a WMO-led long-term research programme addressing climate change[17]), and established a series of workshops that were run by UNEP/ICSU/WMO and held in Villach, Austria in 1980, 1983 and 1985 (Boehmer-Christiansen, 1994a). The 1985 Villach meeting is particularly noteworthy for being an early – if not the first – time that an international panel of scientists came out with a consensus statement on climate change: "in the first half of the next century a rise of global mean temperature could occur which would be greater than any in man's history (WMO, 1985)".

Following the Villach 1985 statement, Agrawala suggests that "climate change had truly 'arrived' both in the news media and the international policy agenda (Agrawala, 1998a: 608)". The ascendancy was relatively fast; although climate change had been promoted as a scientific issue throughout the 1970s, the issue claimed much greater attention from the policy and media spheres in the 1980s (Boehmer-Christiansen, 1994a). Although many factors affected the rise of

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16 http://unfccc.int/essential_background/library/items/3599.php?such=j&meeting=%22The+First+World+Climate+Conference%2C+12-23+February+1979%2C+Geneva%2C+Switzerland%22#beg, accessed December 29, 2011.
17 http://www.wmo.int/pages/prog/wcp/, accessed December 29, 2011.

climate change as a global issue,[18] which Agrawala (1998a) acknowledges, Boehmer-Christiansen (1994a: 158) suggests that at least "part of the answer comes from the skilful dissemination of the Villach message by the scientific community itself". Despite the fact that the Villach conferences occurred under the oversight of UN agencies, the statement was produced by individual scientists and not by official representatives of countries (Jäger, 1992). Scientists may have been spear-heading climate change within UN fora, but there was not any formal means for either vetting their conclusions or for assessing the policy implications of these conclusions at the international level,[19] even if at least some of the scientists were aware that the policy dimensions of their work were becoming increasingly important (e.g. Schneider, 2009).[20]

This void was recognised by numerous actors within the international system, not least the director of UNEP at the time, who played a key role in establishing a precursor to the IPCC, the Advisory Group on Greenhouse Gasses (AGGG).[21] The AGGG was a small group of scientists, many of whom had been involved in the Villach conferences, mandated to advise WMO, UNEP and ICSU on matters pertaining to climate change (Agrawala, 1999). The AGGG organised a key workshop in Bellagio in 1987, which successfully engaged a wide range of policy actors and concluded that increases in air and sea temperatures would need to be held within "tolerable" rates (Ibid.). The results from Bellagio fed into other high-profile climate change events in the mid-1980's, most notably a 1988 NGO-

[18] As discussed in Chapter 1, the "global" framing of the issue is contingent and its framing as such is in itself a worthy focus for analysis (e.g. Yearley, 1996). The analysis of this chapter is, however, as earlier mentioned, is focused on the politics and science more proximal to the establishment of IPCC.
[19] At the national level, the USA was the key nation involved in climate change science, and by the mid-1980s one of the only countries to have conducted formal climate change assessments, having done so from the early 1970s onwards (e.g. Agrawala 1998; Schneider 2009).
[20] Schneider (2009) discusses rifts within the scientific community concerning whether they should also have been discussing the implications of climate change impacts; these began as early as the 1974 GARP meeting in Stockholm (pp. 55-58).
[21] Aside from the activities described below, the AGGG also played a role in organising and disseminating the findings from the Second World Climate Conference, which took place in Geneva in 1990 (Boehmer-Christiansen 1994a), although by this time AGGG had largely been superseded by IPCC.

organized conference held in Toronto (for which several AGGG members also served on the steering committee). The conclusion from the Toronto meeting was even more political than Bellagio: it stated that CO_2 emissions should be reduced to 20% of 1988 levels by 2005. Bert Bolin, an AGGG member at the time, later described this as an "unrealistic ad-hoc recommendation (Bolin, 2007: 48)". Nonetheless, the Toronto statement attracted plenty of media attention, perhaps partially aided by its timing, which coincided with a serious drought in the US Midwest (Jäger, 1992).[22]

After only a few years in action the AGGG disbanded, in part because some of its scientists were weary of the political aspect of their work, and in part because of the emergence of IPCC which proved more suitable for dealing with the emerging scientific and political climate change landscape (Agrawala, 1999). In fact, from nearly the beginning of AGGG's tenure, discussions had been ongoing between UNEP, WMO and a few countries, notably the USA, over the best mechanism for assessing climate change. The USA, which had not been satisfied with the key role played by scientists in the Villach statements (Hech and Tirpak, 1995) and the AGGG (Agrawala, 1998a),[23] was particularly influential, having the most expertise in climate change science but also much at stake economically (Ibid.). The USA ultimately advocated the development of the IPCC as a mechanism for producing international climate change assessments. This was a compromise outcome of complicated domestic American politics, where some agencies viewed the mechanism as a means of "buying time" while others saw it as a necessary starting point for gaining scientific consensus in advance of a global treaty (Hecht and Tirpak, 1995). From the UN perspective,

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[22] The role of adverse weather events in the history of climate change politics is a long one, mirroring some of the evidence presented by interviewees in Chapter 3. The account by Hecht and Tirpak (1995) also identifies severe instances in which adverse weather influenced climate change politics within the US.
[23] Boehmer-Christiansen (1994b: 187) notes that the US State Department believed AGGG represented no more than "free wheeling academics". Moreover, others have suggested that following the experience of the Montreal Protocol for the reduction of Ozone in the atmosphere, in which a small group of scientists were able to dominate the proceedings, the US and WMO were both in favour of a mechanism which would enable them greater control over the science (e.g. Miller, 2004).

many seemed to have shared the idea that there was a need for a "more trustworthy (Bolin, 2007: 49)" assessment mechanism than AGGG, which had a very small membership and was formally quite far removed from policy spheres. Furthermore, in anticipating a global climate treaty, many actors perceived the need to foster international consensus on climate change science through an intergovernmental mechanism:

> …the IPCC was the product of an intensely *political* process within the US and the UN system. The specific purpose for setting it up was also political: to engage governments worldwide in climate change decision-making (Agrawala, 1998a: 617, original emphasis).

Similarly, Stephen Schneider (2009: 125) recalled a conversation with Bert Bolin just before he became the first chairman of IPCC, in which he asked: "Will it be possible to have climate policy without having a scientific group in which various countries of the world have some political ownership?"

Based on its pre-history as described here, the formation of IPCC can be viewed as the product of mutually stabilising relationships between the scientific and the political world. The political world set in motion, and made possible, much of climate science; this in turn perpetuated the need for a global politics and set of institutions to address the findings from climate science. Given the prominent role that climate scientists played, it was not entirely without foundation to suggest that

> ..major well planned research programmes provided the scientific community with the motivation to stimulate public concern and collaborate with environmentalists to attract the attention of politics and policy (Boehmer-Christiansen, 1994a: 146).

It would, however, be somewhat naive to believe that climate scientists could have played such a strong role if the political world was not willing and interested in supporting this topic (e.g. van der Sluijs et al, 1998).[24] Nevertheless, through the relationships described here, the impetus for an *intergovernmental* climate change assessment mechanism gained considerable momentum. Following further discussions over the exact form the mechanism would take, WMO and UNEP invited WMO member governments to participate in IPCC in March, 1988. By November, 1988, IPCC had held its first meeting with the participation of 28 countries (Ibid.).

5.2.2 The IPCC mandate

The IPCC received its formal mandate from UN General Assembly Resolution 43/53 of December 6, 1988.[25] In this Resolution, the UN General Assembly:

> Requests the Secretary-General of the World Meteorological Organization and the Executive Director of the United Nations Environment Programme, through the Intergovernmental Panel on Climate Change, immediately to initiate action leading, as soon as possible, to a comprehensive review and recommendations with respect to:
>
> (a) The state of knowledge of the science of climate and climatic change;
> (b) Programmes and studies on the social and economic impact of climate change, including global warming;
> (c) Possible response strategies to delay, limit or mitigate the impact of adverse climate change;
> (d) The identification and possible strengthening of relevant existing international legal instruments having a bearing on climate;
> (e) Elements for inclusion in a possible future international convention on climate

Resolution 43/53 legitimised and reinforced work that was in fact already underway at IPCC. By the time the IPCC's first meeting ended in November, 1988, IPCC had its formal intergovernmental status, a small bureau, a secretariat organised by WMO, and a structure based around three working groups (Agrawala, 1998b). The focus of the working groups, as well as the chairs for each group, were negotiated in advance of the meeting and approved

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[24] In her account, Boehmer-Christiansen (1994a, 1994c) goes further to suggest that the research community pushed the climate change agenda within policy circles to secure further research funding in this area. Others have pointed out that Boehmer-Christiansen's assessment of the role of scientists appears somewhat exaggerated (Shackley & Skovdin, 1995).
[25] http://www.un.org/documents/ga/res/43/a43r053.htm, accessed December 30, 2011.

without much discussion (Bolin, 2007). The three working groups, which have similar foci today, were Working Group 1 (WGI), which assesses the physical science aspects of climate change; WGII, which assesses impacts of climate change on socio-economic and natural systems, as well as options to adapt to climate change; and WGIII, which assesses options for mitigating climate change. The demarcation of the three working groups offers insight into one of the aspects of IPCC that make it a particularly interesting organisation to study: the ways in which it has attempted (and sometimes struggled) to cope with its dual science-political nature. As Miller (2004: 60) commented, the demarcation between WGs

> demonstrates early efforts to distinguish between the scientific and political aspects of the panel… Over time, however, this separation proved inadequate in pragmatic terms as the IPCC grappled with the day-to-day problems of formulating and carrying out its work plans.[26]

Aside from the way in which it reinforced a particular division of labour between the different Working Groups, the IPCC mandate is noteworthy because it specifically asks IPCC to provide inputs into a future international climate convention – which would eventually become the 1992 UN Framework Convention on Climate Change (UN FCCC)[27] – further demonstrating the close coupling between science and policy that have existed at IPCC since its inception.

5.2.3 IPCC processes: seeking credibility and legitimacy

Sitting as it does at the interface of highly uncertain global climate change science and highly contentious global climate change politics, to suggest that the IPCC mandate is challenging would be something of an understatement. By the time IPCC received its formal mandate in 1988, however, there were no clear

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[26] Bolin (2007: 50) similarly concedes that "The choice of chairmen of the IPCC and its working groups reflected how both scientific competence and political considerations played a role".

[27] http://unfccc.int/essential_background/items/6031.php, accessed January 3, 2012.

instructions on how to pursue it and the IPCC did not have much time for contemplation: IPCC had been asked to complete its first assessment in time to present it to the 1990 UN General Assembly and to gain momentum for a climate convention, which would become the UN FCCC at the UN Conference on the Environment and Development in Rio in 1992. Thus IPCC started its activities in a rather ad-hoc manner. Bolin confesses:

> It was clear to the leaders of the IPCC that we had to develop our own procedure for how to achieve the task that had been given us. During the first couple of years we formally followed the WMO procedures when in doubt. The lack of more precise rules of procedure for a task that was going to be rather different from ordinary WMO activities gave the IPCC great flexibility in handling matters and could be exploited to the advantage of the assessment process, but care had to be exercised. It gradually became apparent, however, that we had to become more strict and professional in our work… (Bolin, 2007: 50-51)

Becoming more strict and professional was, no doubt, necessary to shore-up IPCC's scientific credibility and political legitimacy. One way of achieving this has been through attempts to "de-politicise" the IPCC. A rhetorical shift has gradually placed more emphasis on the "scientific" nature of the IPCC. In the run-up to Rio, for example, developing countries were suspicious of the IPCC's role as a negotiating body for the FCCC, which led to the establishment of the separate International Negotiating Committee (INC) (Agrawala, 1999). The tactical division between the "scientific" IPCC and "political" INC "had a number of important stabilizing effects for the climate regime as a whole (Miller, 2004)", of which the most immediate was a dampening of the criticism IPCC had been receiving from developing countries. There were additional reasons to tweak the IPCC's rhetorical science/politics boundary. IPCC had encountered very visible resistance to its assessments in advance of Rio (e.g. Singer, 1992). Moreover, Article 2 of the UN FCCC placed responsibility on IPCC to help define "dangerous anthropogenic interference", meaning the IPCC work had the possibility to become even more contentious given the "hybrid" nature of such an assignment (and one that very clearly involves value judgements (IPCC, 2001b, Schneider and Mastrandrea, 2005)).

Aligned with its desire to emphasise its "scientific" nature, the IPCC has pursued an ever-evolving series of tweaks and refinements to its structure and processes, significantly via major revisions implemented in 1993 and 1999. Amendments to IPCC procedures have included additional guidance on selecting authors and conducting peer-review, an attempt to harmonise such procedures across the Working Groups, a minor revision of the focus of WGII and WGIII, the addition of review editors, and the establishment of Technical Support Units for each WG, with the purpose of acting as buffers between IPCC "experts" and special interests who would want to influence the reports (Agrawala, 1998b, Bolin, 2007, Hulme and Mahony, 2010, Miller, 2004, Skodvin, 2000). Behind each of these adjustments (in addition to countless other innovations, such as the IPCC's early establishment of the "Summary for Policymakers" report which accompanies formal assessments) lie clear objectives: to maintain scientific credibility, to maintain policy-relevance and foster ties with policy-makers, and to ensure political legitimacy across governments.

Despite the many procedural changes IPCC has implemented, it has also sought to retain some operational freedom. As Edwards and Schneider (2001: 227) observed:

> By the standards of many political organizations, its formal rules of governance are not very extensive. They are also not very specific. The rules purposely leave undefined the meaning of key terms such as "expert" and important processes such as "taking into account" comments.

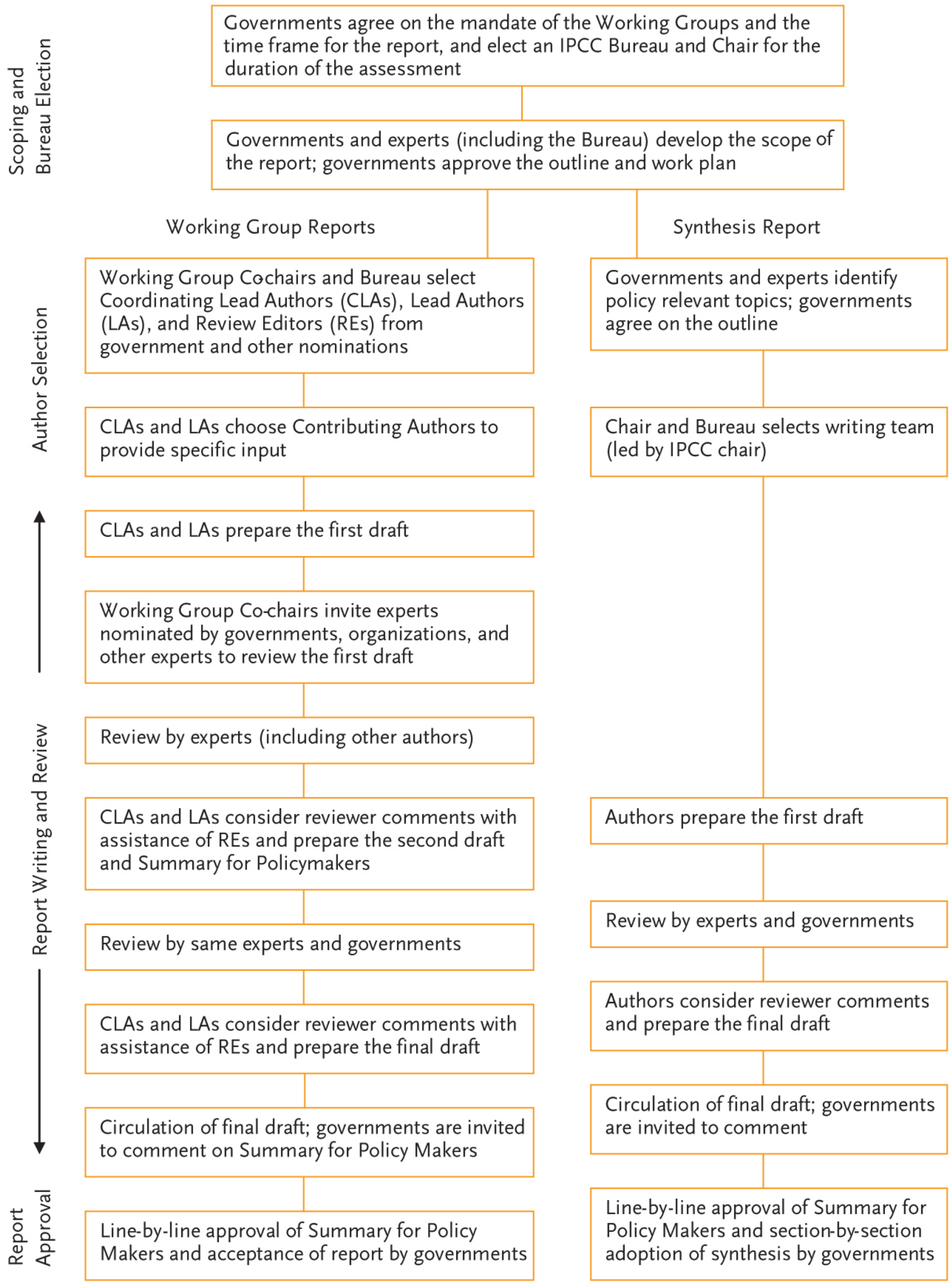
Perhaps partially because of this flexibility, much of the IPCC process has come under intense scrutiny from academics, lobbyists, policy-makers and national governments. In addition to critiques about the IPCC review process, the nature of IPCC consensus and the manners in which uncertainty have been addressed have also received vast amounts of attention (e.g. Petersen, 2011, Swart et al., 2009, Shackley and Wynne, 1996). A brief examination of the current procedures for IPCC assessments (as currently outlined in Appendix A of the IPCC document *Principles Governing IPCC Work*[28]) identifies the multiple and overlapping influences that IPCC writing teams and national governments have on the production of IPCC assessments (Figure 4).[29]

In the remainder of this sub-section, the general aspects of this process will be described, while in the next section, 5.3, the ways in which certain processes, namely the selection of authors and peer review, have specifically influenced the IPCC assessments of human health, will be examined.

[28] *Principles Governing IPCC Work* was adopted in 1998 and subsequently revised in 2003 and 2006, and must be reviewed (and amended if deemed necessary) at least every five years. As the IPCC states: "Comprehensiveness, objectivity, openness and transparency: these are the guiding principles governing IPCC work". See: http://www.ipcc.ch/pdf/ipcc-principles/ipcc-principles.pdf, accessed January 5, 2012.

[29] After having been adopted in 1999, the IPCC assessment process was amended twice in 2003, once in 2008, and twice in 2011. See: http://www.ipcc.ch/pdf/ipcc-principles/ipcc-principles-appendix-a-final.pdf, accessed January 5, 2012.

Figure 4. The IPCC assessment process [30]



[30] This Figure was kindly provided by the InterAcademy Council. It is Figure 1.2 in IAC (2010).

i) The scoping meeting

The very beginning of the IPCC assessment process consists of an invitation-only scoping meeting between IPCC and governmental focal points. During the meeting, the objectives and outline of the upcoming Assessment are discussed; thereafter a scoping document is prepared and submitted to governments, and then a Plenary session of IPCC (consisting of government representatives) needs to approve the scoping paper for each IPCC Working Group.[31] Given that the outline of the entire IPCC assessment must be approved by governments, the plenary sessions are particularly important in shaping IPCC Assessments and also in ensuring that they remain policy-relevant:

> the outline, the broad overview … it's actually driven by a demand of policymakers (**R2**).
>
> there are governments that have very strong opinions about what should be done and what should not be done…There were certainly lots of rumours, that I personally can't confirm but I believe were true, that in the Fourth Assessment Report, the US government tried to get one of the chapters pulled because they didn't like what the chapter was doing … in the end they did not succeed but there are these tensions because you do have to have approval by the governments (**R6**).

Thus any national government might lobby to have content added or removed, potentially altering the focus of the Assessment, but as the one interviewee suggested, countries rarely succeed in doing so if acting alone (**R6**).

ii) Nomination and selection of experts

Once the outline of an Assessment is agreed upon, IPCC once again solicits governments for a list of nominated candidates to be Coordinating Lead Authors, Lead Authors, Contributing Authors, Review Editors and Government Focal Points. The co-chairs of IPCC Working Groups, the Technical Support Units, and the chair and vice-chairs for each WG select the Convening Lead Authors (CLAs), Lead Authors and review editors, based upon the required

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[31] For further details about how this took place for the currently underway IPCC AR5, see: http://www.ipcc.ch/meeting_documentation/workshops-experts-meetings-ar5-scoping.shtml, accessed January 8, 2012.

expertise but also trying to ensure geographic and gender diversity among the writing teams. All selected are unpaid by IPCC throughout the process.

The selection of authors for IPCC assessments has some obvious problems. One relates to their objectivity, which is potentially compromised for two reasons – first because it is governments who nominate "experts", and second because it is highly doubtful that a scientist known to have ambivalent views towards climate change would be selected by IPCC. As concerns the former, one interviewee, well known to be hostile to CCH research, noted:

> **R13**: The intergovernmental [in the acronym IPCC] as I said is intergovernmental … It's not saying the "scientific", it is "governmental". Nobody hides that, and as I said it is governments who make the nominations, so obviously, Mr. Blair's government would not have nominated me, but Mr. Bush's government did - to my embarrassment. So the government makes the nominations and some sort of committee, which is not very clear, make the selections and I'm not really sure of the details to be really honest … [but] everybody knew that I wasn't going to be a lead author.

It does seem to be the case that there is not a high level of transparency in the final selection of writing teams. However, it is important to note the IPCC requirement for diversity in writing teams is driven by its desire to ensure broad legitimacy and governmental buy-in to its outputs: "there has to be a balance from countries and genders ... [to avoid] too many white men (**R8**)". As another interviewee noted:

> **R15:** That was also of course the dilemma. How do you keep intact the same scientific integrity and have enough diversity… in the team as well? That was not always easy…
>
> …you are looking for authors also from African region, from South America, from Asia and in some cases that is very fine, very easy. But if you look at climate change science … it is dominated by the US, by parts of Europe, maybe Germany, the UK and the Netherlands and, to some extent, Scandinavian countries. So, there is not much very high standard climate change science, especially in the early days, going on in Africa for example …

Thus the final composition of writing teams is inevitably subject to criticism about their level of expertise as well as political criticism about the scope of representation. Moreover, as suggested above, the final composition of writing teams is likely to be philosophically aligned with IPCC on the major substantive issues:

> **R6:** The secretariat then has these huge long lists of possible people. The co-chairs for the working group and the technical service unit work with the secretariat to identify the convening lead authors. They then turn around and work... Once you've chosen your convening lead authors, you then have a very long list of possible authors. And there is some fairly extensive process of saying, *"Who's your ideal team that you'd like to have?"* We also, across the working group, need geographic balance and, to the extent possible, we need gender balance... and so then there is working with the convening lead authors, the secretariat, the TSU, the co-chairs, to say "what are these author teams is going to look like? What are the constraints to the number of people?" (emphasis added)

Given such a selection procedure, it would be very difficult indeed to satisfy all governments that they have been adequately represented. As concerned the WGII report for IPCC AR4, for example:

> **R6:** There were 12 US authors but if you count up how many European authors … there's a lot more than 12. So there's always this tension where people point at the US and say you have more authors than any one country, but then Europe must have had 40 authors … and the developing countries often feel they are underrepresented.

Of all selections, the CLA is perhaps the most important one, as they will play a role in guiding contributing authors and also play a key role in defending the conclusions from their chapters in plenary sessions. As one interviewee stated with regards to the review process and in particular the plenary sessions (which negotiate the final text for the SPM):

> **R6:** …this is where it's critically important that you have very strong convening lead authors, because when a country raises an objection the CLA has to be able to come back.

Once writing teams are assembled, they meet on a regular basis as they work towards the preparation of drafts of the full report as well as the Synthesis Report, which includes a Summary for Policymakers (SPM) and a longer, more detailed report. Authors are instructed to draw upon the international peer-reviewed literature as extensively as possible as they prepare their reports, but are allowed to draw upon grey and unpublished literature as well.[32] Importantly, writing teams must also address the comments from reviewers, and they are instructed to identify areas in which consensus does not exist. As the IPCC *Principles* state: "It is important that Reports describe different (possibly controversial) scientific, technical, and socio-economic views on a subject, particularly if they are relevant to the policy debate (IPCC, 1999)".

iii) Peer review

The peer review process has been, and continues to be, one of the most discussed elements of the IPCC assessment process (e.g. Agrawala, 1998b, Edwards and Schneider, 2001, Berkhout, 2010, Hulme and Mahony, 2010, Yearley, 2009). Insights on how this process has influenced the CCH content of IPCC Assessments will be further discussed in 5.3; here, the general process and some of its major discussion points will be introduced.

Peer review for IPCC Assessments begins after writing teams have finished their 1st order drafts. Once completed, the 1st order draft is sent to a wide range of experts, which include scientists with expertise in areas covered by the Assessment, experts nominated by their national governments to contribute as authors or expert reviewers, and experts nominated by other organisations. To facilitate this process, IPCC introduced review editors in its 1999 revision of procedures. Their role is to help identify reviewers, to ensure that all comments are considered, and to advise authors on how to handle controversial issues

[32] The 1999 revision to IPCC rules accounted for the possibility of including non-peer review literature. In the wake of *Climategate*, Hulme and Mahony (2010) remark that is was "prescient" that Skovdin (2000) had anticipated that this could eventually cause credibility problems for IPCC.

(Edwards and Schneider, 2001). After taking the expert review comments into consideration, a 2nd order draft is produced and subjected to a joint expert-governmental review. Once again taking into account all comments, a final draft is produced, alongside the Synthesis Report. These drafts are then subject to acceptance and approval at WG plenary sessions.

One of the most obvious problems for IPCC peer review has related to its scope and its perceived impartiality. As mentioned above, peer reviewers in IPCC include IPCC authors and government-nominated "experts". Given the vast number of scientists involved in assessments, one consequence is that there are "virtually no 'peers' … not already within the IPCC (Yearley 2009: 396)". Despite this predicament, Edwards and Schneider (2001: 225) maintain that peer-review is the principal mechanism of "independent self-governance" at IPCC – designed to establish credibility across its scientific and political stakeholders. In addition, the volume of comments received places a large burden on writing teams, who have to consider all of them.

It is important to note that the vast number of comments that IPCC receives come not only from scientists and policy-makers and governments, but also special-interest groups and lobbyists. Thus part of the work of IPCC writing teams is boundary-related work deciphering whether they consider comments to be "scientific" or "political":

> **R2:** I mean not all people commenting are really scientists … you have also governmental comments. So you might also have statements like "but this is nonsense" or … "you know this is not how I would say it". Or you have one government say "in our country it is like that and I want you to state it like that"

> **R6:** … We had more than 800 perhaps a thousand comments... and the comments come from other scientists, so they are true peer review, they come from any interested member of the public, they come from policy makers, they come from people with particular perspectives, and you have to write a written response to each and every comment ... It's much more strenuous than getting something for peer review for a journal.

Given this, one of the major issues in the IPCC peer review process relates to what and how the writing teams determine which comments necessitate the greatest consideration. Significantly, writing teams have flexibility, as Edwards and Schneider have noted:

> Chapter authors are required to "take into account" all comments, although the meaning of this phrase is deliberately left vague. Given the volume of commentary and the many duplicate and *irrelevant* comments received, responses may be no more than a couple of words. Yet in aggregate, this extremely extensive peer review process typically leads to hundreds or even thousands of changes, since each document typically goes through several drafts (Edwards and Schneider, 2001: 235; emphasis added).

Edwards and Schneider are correct to state that this style of peer review is extremely extensive and that it consults to far broader range of actors than is normally the case for a scientific publication. It may also be true, from the perspective of IPCC and prominent climate scientists like the late Stephen Schneider, that this process has "created a fairer, more thorough, and hence more powerful method for reaching consensus on the knowledge required for good public policy (2001: 245)". Yet, particularly in light of the events surrounding Climategate (5.1), their account spends too much time defending the IPCC peer review process instead of also considering how structural and subjective factors influence the way that peer review comments are determined to be *relevant* or *irrelevant*. This issue will be further discussed in 5.3 and 5.4. For the time being, it suffices to reiterate that the IPCC's main institutional response to address this has been the introduction of review editors, whose role is to support and guide IPCC authors to follow IPCC procedures (IPCC, 1999). Nonetheless, the problem of subjectivity remains:

> If these writing teams are dominated by opinionated experts holding one particular viewpoint, then, conceivably, they could get away with ignoring some or a majority of all critical review comments (Agrawala 1998b: 626).

Given the intense scrutiny that each IPCC Assessment faces, the desire to mitigate bias is obvious – but it is not obvious that *any* peer review process would be capable of eliminating subjectivity.

iv) Approval, acceptance and adoption of IPCC reports

The terms "acceptance", "adoption" and "approval" have somewhat different meanings at IPCC (IPCC, 1999). "Acceptance" by a WG plenary suggests that a report is balanced and comprehensive, but has not been subject to in-depth discussion. This applies to the full-length assessment chapters. "Adoption" implies "section-by-section" endorsement, and applies to the longer report of the IPCC Synthesis Report. "Approval" means that the text has been subject to line-by-line discussion, and this applies to the Summary for Policymakers (SPM) component of the Synthesis Report (Alfsen and Skodvin, 2010).

Plenary sessions focus on the Synthesis Reports, typically requiring 5-7 days of negotiation to produce the final version. The first focus of the WG plenary involves line-by-line negotiation over the contents of the SPM. After the provisional approval of the SPM, the plenary session will review the longer report of the Synthesis Report section-by-section, ensuring that it is consistent with the final version of the SPM. Once there is agreement on this, the SPM is approved and the longer report is adopted. Approval of the SPM implies acceptance of the full report.[33] The accepted and approved reports then proceed to the full panel plenary, where the contents of reports can no longer be changed, and where reports receive final acceptance (IPCC, 1999).

The plenary sessions are the first and most important step towards the political acceptance of IPCC Assessments (Alfsen and Skodvin, 2010) and thus they are highly contested. It has been suggested that the plenary negotiations resemble "a fox-trot performed by a drunken couple: one lurch forward, followed by a

[33] As IPCC states: "Approval of the Summary for Policymakers at the Session of the Working Group signifies that it is consistent with the factual material contained in the full scientific, technical and socioeconomic assessment or Special Report accepted by the Working Group (IPCC, 1999)".

sideways stagger, then a stumble backwards (Agrawala, 1998b: 627)". In these sessions, IPCC writing teams and national governments negotiate over the final content of the SPM:

> **R6:** So it's the summary for policymakers and the synthesis report where the governments try to make changes to reflect their political realities. Sometimes they're successful, sometimes they're not. The chapters themselves they don't touch.

Although there tend to be more government officials than scientists at WG plenaries, it has been suggested that "scientists still have a significant amount of control over the documents (Alfsen and Skovdin, 2010: 6)". One interviewee substantiated this: "And so the plenary is quite contentious because the authors don't want to have anything changed … and by and large the authors have prevailed (**R6**)".

5.2.4 Summary

The IPCC is easily the most visible organisation involved in conducting policy-relevant assessments of climate change science. The co-product of a wide range of political and scientific forces, IPCC has a unique mandate and set of processes that have been designed to balance scientific credibility with political legitimacy. As has been described here, political and scientific forces jointly shape the production of IPCC assessments, but there remain many opportunities for the IPCC secretariat and its selected writing teams to influence the final reports. The next section will analyse how the IPCC has treated CCH throughout its four assessments.

5.3 CCH through the IPCC

Currently working on its Fifth Assessment Report, IPCC has released four previous Assessment Reports (Table 5) in addition to a series of supplemental reports published in 1992 for the Rio Earth Summit and a range of Special Reports.[34] In the First Assessment Report (FAR), there was very little content focused on the health impacts of climate change. This may be in part because the impacts community was not nearly as cohesive or prominent as the climate change science or policy communities by the time IPCC was established (e.g. Agrawala, 1998a, Bolin, 2007). As described earlier (3.3.4), the CCH community took advantage of this void, working alongside WHO to build the case for more expanded roles of CCH in subsequent IPCC Assessments. Indeed, many familiar members of the CCH community have held prominent roles in these assessments (Table 6). In the following subsections, the principal CCH findings, focused on vector-borne diseases as presented in the separate assessment reports, will be discussed.

Table 5. The IPCC Assessment Reports

	Abbreviation	Year Published
First Assessment Report	FAR	1990
Second Assessment Report	SAR	1995
Third Assessment Report	TAR	2001
Fourth Assessment Report	AR4	2007
Fifth Assessment Report	AR5	2014 (expected)

[34] http://www.ipcc.ch/publications_and_data/publications_and_data_reports.shtml, accessed January 12, 2012.

Table 6. Composition of IPCC Human Health Chapter Writing Teams ***(*indicates that the author was interviewed in this research)***

Report	Coordinating Lead Author	Lead Authors (selected)	Contributing Authors (selected)	Review Editors
SAR	-Tony McMichael* (Australia)	-Paul Epstein (USA) -Andrew Haines (UK) -Jonathan Patz (USA)	-Roy Anderson (UK) -Sari Kovats* (UK) -Pim Martens* (Netherlands) -Alistair Woodward* (New Zealand)	n/a
TAR	-Tony McMichael* (Australia) -Andrew Githeko (Kenya)	-Duane Gubler* (USA) -Andrew Haines (UK) -Sari Kovats* (UK) -Pim Martens* (Netherlands) -Jonathan Patz (USA)	-Kris Ebi* (USA) -Elisabet Lindgren* (Sweden)	-Alistair Woodward* (New Zealand) -Ulisses Confalonieri (Brazil)
AR4	-Ulisses Confalonieri (Brazil) -Bettina Menne* (WHO Regional Office for Europe / Germany)	-Kris Ebi* (USA) -Sari Kovats *(UK) -Alistair Woodward*(New Zealand)	-Simon Hales* (New Zealand)	- Susanna Curto (Argentina) - Tony McMichael* (Australia)
AR5	-Alistair Woodward* (New Zealand) - Kirk R. Smith (USA)	-George Luber* (USA) (Emergent risks Chapter) -Diarmid Campbell-Lendrum (WHO) -Rainer Sauerborn (Germany)		-Andrew Haines (UK)

5.3.1 The First Assessment Report (FAR)

As noted above, the IPCC FAR contains very little content on health. It forms only a small component of a broader chapter addressing the impacts of climate change on human settlement and migration; the energy, transportation and industrial sectors; air quality; and changes in ultraviolet radiation. None of the authors were core members of the CCH community.

FAR does however note that should global warming lead to changes in rainfall and temperature, then "the seasonal and geographical abundance" of vector species, such as mosquitoes, could change; this could mean a northerly expansion in the Northern Hemisphere and a southerly expansion in the Southern Hemisphere (Rouviere et al., 1990: 6.2.6). For tropical regions, precipitation increases could drive the incidence of malaria, schistosomiasis and dengue, all of which, the report notes, “have the potential for increase and reintroduction in many countries (Ibid.)”.

Intriguingly, the accompanying SPM document offers a somewhat more alarmist take on the issue:

> Changes in precipitation and temperature could *radically* alter the patterns of vector-borne and viral diseases by shifting them to higher latitudes, thus putting large populations at risk (Tegart et al., 1990; emphasis added).

This text would set the tone for future IPCC Assessments, which would be conducted under much more scrutiny and controversy.

5.3.2 The Second Assessment Report (SAR)

The IPCC Second Assessment Report (SAR) is the first report published after the establishment of UN FCCC in 1992. As discussed in 3.3.4, the SAR would also mark the beginning of the involvement of the CCH community, who had gradually gained prominence with WHO and had used this opportunity to directly appeal to IPCC to include a chapter on human health.

With Tony McMichael as the Coordinating Lead Author and a host of other CCH proponents involved as authors (Table 6), the health chapter in SAR offered a much more comprehensive analysis than did FAR. It was no less bold in its pronouncements, claiming in the SPM document: “Climate change is likely to have wide-ranging and mostly adverse impacts on human health, with significant loss of life (IPCC, 1996: 11)”. The health writing team divided the possible health impacts from climate change into those with direct effects (e.g. mortality due to heat waves) and indirect effects, which focused particularly on vector-borne diseases. Malaria, dengue, yellow fever and a few food- and water-borne diseases are all highlighted, but the focus is clearly on malaria.[35] As the SPM would further state:

> Projections by models…indicate the geographical zone of potential malaria transmission in response to world temperature increases at the upper part of the IPCC-projected range … would increase from approximately 45% of the world population to 60% of the world population by the latter half of the next century. This could lead to potential increases in malaria incidence (on the order of 50-80 million additional annual cases, relative to an assumed global background total of 500 million cases), primarily in tropical, sub-tropical, and less well-protected temperate-zone populations (Ibid*.:* 12).

Further details are provided in the full health chapter where the discussion on malaria is more nuanced. The chapter notes the historical contraction of malaria in the developed world and argues that the existing public health resources in these countries makes "reemergent malaria unlikely (Ibid.: 572)". It does however argue that malaria is most likely to extend its spread in tropical countries, "particularly in populations currently at the fringe of established endemic areas (Ibid.)". One of the possibilities identified here is highland malaria:

> ...it is a reasonable prediction that, in eastern Africa, a relative small increase in winter temperature could extend the mosquito habitat and thus enable falciparum malaria to reach beyond the usual altitude limit of around 2,500m to the large, malaria-free, urban highland populations… (Ibid.: 574).

It is significant that these conclusions on malaria, which underpin the SPM, are primarily based upon early research produced by the same authors involved in the IPCC writing team. The statement about highland malaria, for example, is supported by only one citation, which turns out to be an essay in Lancet penned by Haines, Epstein and McMichael on behalf of an international expert panel organised by WHO to discuss possible ways to monitor the health impacts of

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[35] In the full chapter, Malaria is the only disease deemed to be "highly likely" to have its distribution ranges altered by climate change, while Dengue, Yellow Fever, Schistosomiasis and Onchocerciasis (River Blindness) all deemed "very likely" (McMichael et al., 1996).

climate change (Haines et al., 1993).[36] This essay identifies highland malaria as one issue worth monitoring for the early impacts of climate change, but it does not present any research findings about the topic. Similarly, the malaria projections are based upon the research produced by Martens and co-authors, including Tony McMichael, discussed at length in 4.3.1 (Martens et al., 1995a, Martens et al., 1995c).

To be fair, the chapter also provides a text-box discussion of the ins and outs of biological models for assessing the links between climate change and malaria. It is fairly nuanced, providing many caveats: "As a highly aggregated model, it does not take account of local environmental-ecological factors, and it therefore cannot be regarded as a source of precise projections (McMichael et al., 1996: 573)". Nonetheless, despite highlighting the model limitations and the need to "validate" the models, it is argued that "such models… provide indication about the likely impact of climate change on the potential transmission of vector-borne diseases (Ibid.: 574)".

It must also be pointed out that when SAR was under preparation, there were relatively few climate change-VBD publications, and most of these were not based upon models. This may partially excuse the inconvenient truth that much of the literature cited on this topic was self-referential. The IPCC authors were, for the most part, citing their own research – and as discussed above, in some cases they were merely citing their earlier speculations.

It is possible that the strong representation of the CCH community in the writing team prevented them from seriously considering alternative perspectives:

> **R15:** …we had not that much discussion in the whole process because again I think the climate change community already knew it [the science].

[36] Recall 3.3.4 for further discussion on the involvement of the CCH community in early WHO activities addressing CCH.

Even if the community already knew and agreed upon all of the literature, this did not necessarily mean that no alternative interpretations were available. Although instructed to represent differences of opinion, the authors did not represent the views from one of the few prominent papers that had been published about climate change and epidemiological modelling before 1995. It would have been quite reasonable to at least refer to an argument carefully articulated by David Rogers and Michael Packer in a 1993 *Lancet* article entitled "Vector-borne diseases, models, and global change":

> It is probably correct to assume that a vector now confined to the tropics will spread into more temperate regions if global warming occurs ... but it is much less certain that the diseases they carry will eventually be as prevalent in the newly invaded areas as elsewhere (Rogers and Packer, 1993: 1283).

That this is not even referred to is even more striking when noting that the Rogers and Packer paper is in fact cited in IPCC SAR, but only to support a claim that the distribution of tsetse flies, the main vector of African trypanosomiasis, could be significantly altered by small temperature changes (McMichael et al., 1996: 574). This was, moreover, a curious use of the citation, given that Rogers and Packer themselves self-deprecatingly suggest of their model:

> Such predictions should not be used to frighten public health planners into precipitate action over the possible impact of global warming--on the scale of "lies, damned lies and statistics" the prediction illustrated is slightly to the right of "statistics"--but they can be used to encourage more careful study of how important predictors operate in the biological systems on the ground (Rogers and Packer, 1993: 1284).

Thus IPCC SAR, although somewhat transparent about the limitations of the malaria models and need for model validation, did not carefully discuss the possibility that more fundamental differences of perspective existed in the scientific world. The composition of the writing team, with a strong personality in Tony McMichael as the CLA and with several other CCH proponents involved, appears to have had a significant influence over the way in which the science was perceived. It was also likely influential in negotiating the stronger message found in the SPM.

5.3.3 The Third Assessment Report (TAR)

A familiar cast of characters was involved in TAR. The Coordinating Lead Authors (CLAs) were Tony McMichael and Andrew Githeko, who was a central actor in the controversy surrounding climate change and highland malaria (4.4). Among the Lead Authors were CCH researchers Pim Martens, Sari Kovats, and Jonathan Patz. Yet this time around, two prominent CCH opponents would find their way into the IPCC process. Duane Gubler, then of the US CDC, would be a Lead Author and Paul Reiter, then also of CDC, would contribute to the review process. Further complicating the matter for the CCH proponents would be that many of the climate-VBD controversies discussed in Chapter 4 were well underway, meaning that the writing teams would need to consider a literature vastly larger and more contentious than in previous assessments.

Noting that the science has progressed since the SAR, the SPM document states that many infectious diseases are climate sensitive. As concerns infectious diseases:

> From the result of most predictive model studies, there is *medium* to *high confidence* that, under climate change scenarios, there would be a net increase in the geographic range of potential transmission of malaria and dengue – two vector-borne infections each of which currently impinge on 40-50% of the world population (original emphasis) ... In all cases, however, disease occurrence is strongly influenced by local environmental conditions, socioeconomic circumstances, and public health infrastructure (IPCC, 2001a: 12).[37]

The above statement is accompanied by a footnote which briefly summarises the evidence base supporting such a claim:

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[37] By medium confidence, IPCC means a 33-67% degree of confidence, and by high confidence, IPCC means 67-95%.

> Eight studies have modeled the effects of climate change on these diseases, five on malaria and three on dengue. Seven use the biological, or process-based approach, and one uses an empirical, statistical approach (Ibid.).

It is instructive to assess the IPCC treatment of these eight studies and to contrast it to the statement that found its way into the SPM. As in the previous two IPCC assessments, the discussion in the full report is somewhat more nuanced than the SPM statements. The section on vector-borne diseases starts with a discussion which notes that many infectious diseases are transmitted by climate-sensitive organisms but also carefully alludes to the "many complex and interacting factors (McMichael et al., 2001: 462)" that influence infectious disease transmission. The authors also state that "to date, there is little evidence that climate change has played a significant role in the recent resurgence of infectious diseases (Ibid*.:* 463)". Perhaps for this reason, much of the remaining discussion focuses on predictive modelling studies.

i) Climate change and malaria in TAR

Considering that the climate-malaria controversy was in full-swing by 2001 (recall 4.3 and 4.4) and that IPCC writing teams are requested to reflect controversies in their chapters, the text contained in TAR is surprisingly one-sided.

Noting that the then current resurgence of Malaria was occurring due to a wide range of factors, including policy changes, population growth, drug resistance and deteriorating public health infrastructures, the discussion considers the possible impact of climate changes on malaria transmission in light of socioeconomic circumstances: "in many malaria-free countries with a developed public health infrastructure, the risk of sustained malaria transmission after reintroduction is low in the near term (Ibid.: 465)". However, this time mirroring the CCH community tendency to have somewhat pessimistic visions of the future (4.3.3, 4.5), the chapter also states: "Malaria could become established again under the prolonged pressures of climatic and other environmental-demographic changes if a strong public health infrastructure is not maintained (Ibid.: 465)".

It is noteworthy that the chapter discusses the differences between the biological and statistical modelling approaches. The former are principally represented by various iterations of the Martens models (Martens et al., 1999, Martens et al., 1995a), for which they suggest there has been a "considerable evolution (McMichael et al., 2001: 466)". Similar to SAR, the chapter does admit some of the limitations of models, such as that they had not been validated, that they placed undue emphasis upon temperature at the expense of other ecological variables, and that more smaller-scale models were needed. The discussion, furthermore, concedes that the Martens projections based upon environmental variables do not necessarily mean that increased malaria transmission will occur. In reference to the projection that 260-320 million additional people could live in malaria transmission zones by 2080, the IPCC states:

> This projection, by design, does not take into account the fact that much of this additional population at risk is in middle- or high-income countries where human-imposed constraints on malaria are greatest and where potential transmission is unlikely to be actual transmission (Ibid.: 466).

Nevertheless, the section on malaria modelling concludes that "on a global scale, all biological models show net increases in the potential transmission zone of malaria… (Ibid.)". Additionally, the only figure reproduced in the health chapter is the Martens malaria model (Figure 2, 4.3.1), thereby allotting this research a prominent position in the health chapter.

Whereas the biological models get the bulk of the treatment in the IPCC text, one paragraph is allotted to the statistical approach, which focuses upon the *Science* paper published by David Rogers and Sarah Randolph discussed in 4.3 (Rogers and Randolph, 2000). The treatment of this paper is subtly different in tone. To begin with, as concerns the principal differences between the biological and statistical modelling approaches, the chapter states: "the outcome variable in this model is the number of people living in an *actual* transmission zone, as opposed to a *potential* zone (as estimated by biological models) (McMichael et al., 2001: 466, original emphasis)". The findings – that Rogers and Randolph estimated no significant change in the proportion of the global population that would live in malaria transmission zones by 2080 – are presented but their significance is downplayed:

> This study made the assumption that the actual geographic distribution of malaria in today's world is a satisfactory approximation of its historical distribution prior to modern public health interventions. This assumption is *likely to have biased* the estimation of the underlying multivariate relationship between climatic variables and malaria occurrence because the sensitive climate-malaria relationship in the lower temperature range in temperate zones (especially Europe and the southern United States) would have been excluded from the empirically derived equation. (Ibid.: emphasis added).

The usage of the word "biased" is particularly striking, not because this statement could be said to be fundamentally inaccurate, but because biological models are biased in the other direction. As noted above, this is a point conceded by the authors of the TAR health chapter.

Following a brief description of some models that focused only on mosquito species, the section on malaria and climate-malaria modelling concludes with the following statement:

> …it remains a legitimate and important question to estimate, under scenarios of climate change, change in the extent to which the natural world … would allow transmission of malaria if there were no other human-imposed constraints on transmission (Ibid.).

Without also arguing for more climate-malaria modelling in general, this ending quote privileges modelling work conducted by the CCH community. That this should be the case (just as that the SPM should have stated that with medium to high confidence there will be a *net increase* in areas potentially suitable for malaria transmission) is not completely surprising, given that the author team was dominated by CCH proponents. Yet at least one CCH opponent was in the team, adding some internal friction to the process. As one external observer tells the story, the climate-malaria controversies discussed in Chapter 4 certainly found their way into the IPCC writing process, as did the hard feelings between CCH proponents and opponents:

> **R4:** Now my understanding of what went on in this committee was that it was incredibly controversial and Pim Martens was pushing his line and Duane Gubler wanted to be slightly more circumspect. And Paul Reiter had already had a run-in with Paul Epstein and really didn't want [IPCC] to push the global warming bandwagon too far. He felt they [CCH proponents] were pushing it too far. And I got wind of all this, I can't quite remember how. So I sent a draft of our *Science* paper... to Duane Gubler on the committee. And he circulated it.

This paper, contesting as it did the CCH proponents' "orthodox" position, may have been welcome ammunition for Duane Gubler:

> **R4:** You see before we sent him the paper the draft of the IPCC3 [TAR] was saying that the balance of evidence is that malaria is going to extend its range fantastically in the future. Duane was unhappy with this conclusion based entirely on Martens' work but Duane himself wasn't able to nail why it was wrong or to produce an alternative. So when we sent Duane our *Science* paper he now had something that at least was a counter argument to Pim Martens.

Formally, the IPCC may not have been obliged to consider this paper, because there are cut-off dates for the peer-reviewed literature:

> **R4:** … members of the committee actually tried to throw out our *Science* paper from consideration because there was a cut-off date beyond which IPCC3 [TAR] wasn't supposed to take any notice. It reviews the literature up to a certain date and not beyond.

To their credit, then, the IPCC writing team ultimately did incorporate the *Science* paper into the Health Chapter. Nonetheless, as described above, the treatment of this paper is somewhat different than the treatment of the biological models.

It was earlier mentioned that in the SPM there was a footnote referring to the body of evidence as seven biological models and one statistical model. This statement is, from the perspective of the statistical modellers, not sufficient justification for privileging biological models:

> **R17:** ... I'm not quite sure how they finessed around that problem but if you read the IPCC3 [TAR]... they still state that it seems likely that malaria will expand its range globally in the future...
>
> ...and the footnote says: 'The balance of evidence is eight publications in favour of a great increase in malaria versus one against.' It was a ratio of eight to one. So they were trying to say, 'Look there's a balance but in general the evidence is in favour there's going to be a huge increase.' ...But our immediate response to that was to say, 'Well actually look at those eight papers'. They're all by Pim Martens. They all say the same thing in eight different journals. It's not eight different views, it's one view repeated eight times.[38]

Indeed, the TAR discussion on malaria references numerous different papers authored or co-authored by Martens. As in SAR, this offers evidence that the IPCC writing team felt most comfortable citing its own research. Furthermore, although impossible to verify, it would appear that the internal controversy over how to represent the climate-malaria played out in favour of CCH proponents, given their majority representation in the writing team. Although the text in the technical chapter is more nuanced than the SPM, it does not appear to fairly represent the full scope of the climate-malaria controversy.

ii) Climate change and highland malaria in TAR

Andrew Githeko was one of the two CLAs for the health chapter in TAR, and as discussed in 4.4, he has also been one of the main actors in the controversy surrounding highland malaria. It is therefore not particularly alarming that TAR addresses this topic, although it is noteworthy that highland malaria is not

[38] The interviewee had the numbers mixed up. As cited earlier, the footnote says there are seven biological models and one statistical model for malaria and dengue combined. The IPCC footnote also does not specify that the biological models are "for" and the statistical model "against", but the interviewee's comments here nonetheless suggests that it is somewhat disingenuous to list the number of models because different research papers published by the same constellation of authors are likely to have similar conclusions.

referred to in the SPM. However, the text in the more detailed health chapter states: "Future climate change may increase transmission in some highland regions, such as East Africa (McMichael et al., 2001: 464)". This is accompanied by a text-box entitled "Have recent increases in highland malaria been caused by global warming?" However, the text within this box is in fact much more balanced than the aforementioned statement, suggesting the various possible and contradictory explanations that had been suggested to explain the increase in highland malaria, and noting that further research would be required to decipher the role of climate change in the rise of highland malaria. Further aspects of the climate-highland malaria controversy (4.4) are not alluded to in this text box, but it should be noted that the bulk of the publications surrounding this controversy were published after 2001.

iii) Climate change and dengue in TAR

In comparison to the section on malaria, the discussion on dengue is rather limited in TAR. As earlier mentioned, the SPM suggests that with medium to high confidence, there would be a net increase in the geographic range with potential dengue transmission under climate change scenarios. Yet when reading the full chapter, it is apparent that very few modelling studies had been conducted for dengue at the time. Unlike malaria, the concluding statement, rather than advocating the biological modelling approach for examining future dengue spread, stresses mitigating factors for dengue transmission:

> Transmission intensity in tropical endemic countries is limited primarily by herd immunity, not temperature; therefore, projected temperature increases are not likely to affect transmission significantly. Moreover, in subtropical developed areas, where transmission is limited primarily by demographic and societal factors, it is unlikely that the anticipated temperature rise would affect endemicity (McMichael et al., 2001: 464.).

This message is philosophically very different to the message from the section on malaria, which essentially prioritised the opposite – paying attention to the role of temperature and other climate variables *in spite of* the mitigating variables. That this was the case for dengue could be due to two factors. First, as mentioned above, there were relatively few papers published on dengue by 2001, and the climate-dengue controversy (4.5) hadn't really heated up by then. Second, Duane Gubler is among the world's most prominent dengue specialists, and he can be generally classified as a CCH opponent. His voice would likely have held significant sway over this section of the TAR text.

iv) On the influence of writing teams

This analysis of the climate-VBD controversy as represented by IPCC TAR reveals a discrepancy between the statements made in the SPM and the treatment of the underlying science as presented in the full health chapter. Whereas the SPM signals a fairly strong and positive association between climate change and malaria and dengue, the text is rather more nuanced. That is not to say, at least for malaria, that the text is neutral, and from the discussion above it would seem clear that although the strengths and weakness of the biological and statistical modelling approaches are explained, preference is given to the biological models. Conversely, the text on dengue, limited as it is, appears to give preference to the idea that factors other than climate are most important for dengue transmission.

In both instances, the influence of the particular interests of the various co-authors appears to have weighed in. Moreover, the intense and then emerging controversy over climate-VBD interactions does not appear in the text in its full glory – it is filtered through the perspective of CCH proponents. The CCH opponents involved in the process, either as authors or reviewers, certainly expressed their frustration in it:

> **R21:** Tony McMichael was the principal author on that chapter and there were several lead authors and we all contributed, but at the end of the day it was Tony McMichael that decided on the tone...
>
> ... I think it was 1999 or 2000 and with the first meeting with all of the authors of that IPCC report and there was something like two thousand of us in the room, but I raised the issue of population growth, population density and how

that influences transmission dynamics. And that was off the table. It was simply not discussed because it was a politically charged topic. So they ignored a lot of the demographic factors that were driving this and I think the reason they wanted to ignore it is because it was a counter-argument.

... But in general when people like [us] went to these meetings we were the odd men out, we were very much a minority group.

R13: But anyway, the first draft was among the scientists, and we were writing things down, and [we] both said, "Well this is not true, you've got no evidence for this," … but then it appeared in the draft.

R17: I have to tell you, it was deeply frustrating because they basically, the authors and people who I was reporting to, were very, very reluctant to accept any statement that was contrary to what they already believed... My very strong impression was that the tide of opinion was so much in favour of saying that ... climate change would cause all sorts of bad effects.

Clearly, the influence of personal interests and commitments will play a strong role in any assessment. The IPCC process however has been designed to mitigate this as much as possible, but by 2001 still left plenty of room for judgement. This perhaps cannot be avoided. Even CCH proponents have acknowledged this:

R15: That is actually also one of the ones that you see ... people sometimes have a lot of experience but you feel a kind of certain idealistic motivation underneath which is very fine. I think we all have that but in this complex issue sometimes it is not easy to make distinctions between what you think is real and what you actually have scientific evidence for.

With more CCH proponents than opponents involved in the process, it is more likely that the assessment would be skewed towards their perspective. The review process involves authors addressing those topics that are their particular areas of expertise. As one co-author noted:

R10: ... when the head author of the chapter, it was Tony McMichael at that time, when he got the comments from the reviewers, if there was anything he wanted to ask you know the other authors, he asked us and said 'okay we have got these comments and are there anything you want to add?'

Thus, the drafting of text and the reviewing of comments are both affected by the composition of IPCC writing teams, which are almost certain to include more proponents than opponents of climate change or climate change impacts research.

5.3.4 The Fourth Assessment Report (AR4)

The writing team for the AR4 health chapter is notable for the absence of Tony McMichael and Pim Martens as well as the absence of any notable CCH opponents like Duane Gubler. There were, nonetheless, many contributors from the CCH core group, including the CLA Bettina Menne from the WHO Regional Office for Euro, Kris Ebi, Sari Kovats, Alistair Woodward and Simon Hales.

i) The SPM

The language in the SPM is rather more nuanced in comparison with previous assessments. Among the "projected climate change-related exposures...likely to affect the health status of millions of people", IPCC includes the increased burden of diarrhoeal disease and the altered spatial distribution of some infectious disease vectors. The text about malaria is more cautious than in previous assessments: "Climate change is expected to have some mixed effects, such as a decrease or increase in the range of transmission potential of malaria in Africa (IPCC, 2007)". The SPM for health concludes with a sort of disclaimer, noting that health impacts (whether positive or negative) from climate change would vary from one location to another, and that the factors that shape population health – including education, economic development, health care and public health initiatives and infrastructures – would be critically important as well.

ii) The full chapter: current climate impacts and vulnerabilities

The message from the full chapter is, like the SPM, somewhat more restrained than previous IPCC assessments – the literature cites more widely from both sides of the CCH controversy. The chapter is loosely divided into a section discussing current sensitivities and vulnerabilities and a section addressing future health impacts. As concerns the former, the text generally discusses observed associations between vector-borne diseases and climate change without making any strong, causal statements. Thus, for example, although northern or altitudinal shifts in tick distribution had been observed in Europe, as well as increases in TBE in North America and Europe, "climate change alone is unlikely to explain recent increases (Confalonieri et al., 2007: 403)", citing Sarah Randolph's work as one of the alternative explanations for this upsurge (recall 4.6).

As for dengue, although several studies identified associations between climatic variables and dengue, "these reported associations are not entirely consistent, possibly reflecting the complexity of climatic effects on transmission (Ibid.: 403)".

The longest discussion is focused on malaria, and the text explicitly refers to the longstanding controversy over whether climate change had already impacted highland malaria: "The effects of observed climate change on the geographical distribution of malaria and its transmission intensity in highland regions remains controversial (Ibid.: 404)". The bulk of this text, however, favours the conclusion that climate and climate change have played an important role. Although the research of Simon Hay is presented in which he suggests that increases in malaria incidence occurred "in the apparent absence of climate trends (Ibid.: 404)", substantial doubt is subsequently cast upon it through citing research led by Jonathan Patz: "…the validity of this conclusion has been questioned because it may have resulted from inappropriate use of the climatic data (Ibid.)". A range of studies are then cited demonstrating associations between climate and highland malaria in Madagascar, Kenya, and Ethiopia before concluding by noting another study by Hay critiquing such findings.

It was suggested in 4.7 that a successful challenge on the climate data used in a model would necessarily compromise the perceived validity of the model's outputs. It seems as though, in AR4, levels of "externality" have been removed from the debate on highland malaria. Given that this is an IPCC assessment, it is not entirely surprising: for all the reasons related to how writing teams are selected (5.2.3), it can be expected that the vast majority of IPCC authors accept the premise that climate change is occurring and, therefore, are less likely to be critical of climate change data itself (which is, in any case, the focus of other Working Groups).

iii) Future impacts

The discussion on future impacts and vulnerabilities is, consistent with the aforementioned section and the SPM, also much more muted than in previous IPCC assessments. The subsection devoted to malaria, dengue and other infectious diseases includes, in the very first paragraph, the following statement:

> The magnitude of the projected effect may be smaller than reported in the TAR, partly because of advances in categorising risk. There is greater confidence in projected changes in the geographical range of vectors than in changes in disease incidence because of uncertainties about trends in factors other than climate that influence human cases and deaths, including the status of public health infrastructure (Ibid.: 408).

Very few concrete statements can be found about the possible impacts of climate change on malaria or dengue. The chapter lists a lengthy table summarising key studies and notes many caveats about modelling. For example:

> Models with incomplete parameterisation of biological relationships between temperature, vector and parasite often over-emphasise relative changes in risk, even when the absolute risk is small (Ibid.: 408).

> Malaria is a complex disease to model and all published models have limited parameterisation of some of the key factors that influence the geographical range and intensity of malaria transmission (Ibid.).

Read together, this would appear to be a very technical admission that biological climate-malaria models are not as accurate as previously advertised.

Accordingly, the final claims from AR4 are quite modest. For malaria in Africa, the boldest statement is that an increase in the malaria transmission season could have "important implications for vector control (Ibid.)", but this, citing a publication by Paul Reiter, would not necessarily translate into an increase in the burden of disease due to malaria. The remainder of the discussion on malaria addresses the risks in Europe, Australia, Asia and Central America and is also generally balanced, pointing out, for example, that although climate change might enable a southward expansion of the climatic suitability for vectors in Australia, "the future risk of endemicity would remain low due to the capacity to respond (Ibid.: 408)". This is in clear contrast to TAR, where more pessimistic socioeconomic expectations were emphasised when it noted that malaria could become established in certain countries should public health capacities not be maintained (5.3.3).

Dengue, meanwhile, is given fairly little treatment. Unlike the coverage of malaria in AR4 and dengue in TAR, the literature is not evenly represented. The Hales vapour pressure model (4.5) is the main study cited, and it is simply accompanied by the statement: "It was estimated that, in the 2080s, 5-6 billion people would be at risk of dengue as a result of climate change and population increase, compared with 3.5 billion people if the climate remained unchanged (Ibid.: 408)".

The health chapter in AR4 does not discuss other vector-borne diseases; it refers to chapters on Europe and North America for discussions on tick-borne encephalitis and Lyme disease. These chapters only briefly touch upon infectious diseases. The North American chapter, of which Jonathan Patz was a Lead Author, suggests that the Lyme disease distribution range could shift north, whereas the European chapter contains a fairly balanced but concise discussion of the debate between Lindgren and Randolph (section 4.6) over the various possible causes of an increase in TBE in Europe (Alcamo et al., 2007).

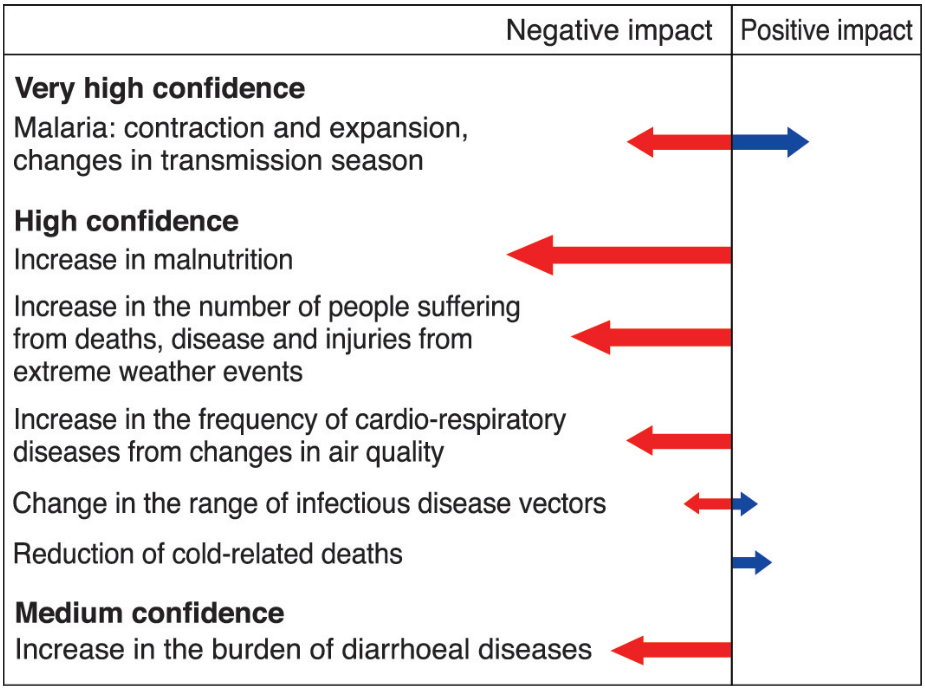
iv) The influence of the AR4 writing team

A general consensus, at least among CCH opponents, is that AR4 achieved a balance hitherto absent in prior IPCC assessments:

> **R4**: IPCC4, the more recent one, achieves a more balanced view than IPCC3.
>
> **Jonathan**: So you feel comfortable with the conclusions from the 2007 assessment?
>
> **R13**: More or less, yes. I can't remember, I read it very briefly, I was so sick of the whole business. But put it this way, we went through two drafts, and the first one I put a lot of suggestions in, and a lot of them were accepted, and then the second one I went over some of the first one, to put them back in again with additional sort of discussion, and I think they accepted some of those.

The discussion in this chapter would appear to slightly confirm these statements, as AR4 at least alluded to the the potential positive as well as negative impacts of climate change on health, evidenced by the depiction of malaria in a heavily cited summary figure produced by IPCC (Figure 5). The assessment nevertheless appears to be skewed towards the potential negative health impacts from climate change, a trend more generally identified for IPCC assessments (e.g. PBL, 2010). One example is dengue, for which we have seen earlier in this section that the research claims were fairly one-sided towards the negative, even in comparison with the claims from TAR. Another (albeit more general) example comes from the chapter on Europe, where the statement in the executive summary that "on balance health risks are very likely to increase" is not adequately substantiated by the more detailed discussion in the main text (PBL, 2010: 69).

Figure 5. Direction and magnitude of change of selected health impacts of climate change, as presented in AR4 [39]



Overall, however, as concerns infectious diseases the message from AR4 is toned down in comparison with earlier IPCC assessments, even if the reasons behind this AR4 are difficult to assess. There are many possible explanations – some unrelated to the state of the peer review literature, which we have already seen has been treated inconsistently across the assessments. One explanation could be that amendments to the IPCC assessment process had had an impact (5.2.3), but then again many of these were made prior to TAR.

Another explanation could be the absence of a CLA with a very strong personality such as Tony McMichael; it has already been noted just how significant the influence of the Coordinating Lead Author (CLA) can be in influencing the peer review and in negotiating the final text for the SPM (5.2.3, 5.3.3). In this regard, it is noteworthy that one Lead Author from AR4 stated the following about the AR4 CLAs:

[39] Available at: http://www.ipcc.ch/publications_and_data/publications_and_data_figures_and_tables.shtml#.T0dgz3mCVGQ, accessed February 22, 2012.

> **R6:** I was a lead author but to be blunt two of us wrote 85% of the chapter because of the CLAs, one was not interested and one was not capable.

Yet another explanation for AR4 could be that CCH opponents, frustrated with the outcome of prior assessments, were more substantially involved in the review process:

> **R2**: And so the controversies got into it… And everybody could who was involved into the review process and the major institutions were involved into this had to make their points on where there was incorrect representation of the science.
>
> So definitely what you would call the climate skeptics I mean the Oxford Group or Paul Reiter, I mean just to mention a few, had ample space and opportunity to comment…

It is at least clear that the volume of the comments that the writing team had to address through the peer review process was enormous and it is important to reiterate that any interested parties could respond:

> **R6:** We had more than 800 perhaps a thousand comments... and the comments come from other scientists ... they come from any interested member of the public, they come from policy makers, they come from people with particular perspectives, and you have to write a written response to each and every comment.[40]

The need to address so many comments introduces, as earlier mentioned, a high degree of subjectivity as writing teams deem some comments significant and others not. The sheer volume of comments, however, could also contribute to a more diluted message, if the writing teams decided to be cautious by addressing as many comments as possible. Here, it is important to note that the climate-VBD message from this assessment could have been *even weaker*, had the final version not benefitted from the deletion of a few choice sentences. In the First-

[40] All comments received and addressed by IPCC for AR4 are in fact available. For the chapter on Human Health, there were 1102 comments on the first-order draft. There were 593 expert comments and 100 government comments on the second-order draft. See: http://www.ipcc-wg2.gov/publications/AR4/ar4review.html, accessed March 3, 2012.

Order Draft, the paragraph on dengue ends with a disclaiming sentence about the Hales model projections:

> This analysis is based on one model; additional models are needed before confidence can be placed on how climate change could affect the incidence and range of dengue.[41]

This statement is shortened in the Second-Order Draft, reading: "additional models are needed to increase confidence in these projections".[42] By the final report, this sentence had been deleted entirely. Similarly, a statement about tick-borne encephalitis appears in both the First- and Second-Order Drafts but not the final report:

> The only other vector borne disease to be mapped and quantified for climate change impacts is tick-borne encephalitis in Europe (Randolph and Rogers, 2000). Increased temperatures are projected to reduce the endemic range of this disease in Europe.[43]

If it is surprising that these statements appeared in the First and Second Order Drafts at all, it is equally surprising that they were subsequently removed. There is nothing in the expert comments for the First- or Second-Order Draft or the governmental comments for the Second-Order draft to suggest that anyone had recommended that these statements be removed.[44] [45] Thus the content of the final version of the chapter appears to offer yet additional evidence that

[41] This statement appears on page 25 of the First Order Draft, available at: http://www.ipcc-wg2.gov/publications/AR4/ar4review.html, accessed March 3, 2012.
[42] This statement appears on page 28 of the Second Order Draft, available at: http://www.ipcc-wg2.gov/publications/AR4/ar4review.html, accessed March 3, 2012.
[43] This statement appears on page 25 of the First Order Draft and page 28 of the Second Order Draft, available at: http://www.ipcc-wg2.gov/publications/AR4/ar4review.html, accessed March 3, 2012.
[44] Searching all expert and government review comments for the words "dengue" and "tick" and "TBE" reveals that the majority of comments about dengue were in fact submitted by Paul Reiter and were critical of any stated role for climate in dengue transmission. All review documents are available at: http://www.ipcc-wg2.gov/publications/AR4/ar4review.html, accessed March 3, 2012.
[45] Paul Reiter, it should be clarified, has been openly critical of IPCC and its processes, even testifying to the UK House of Lords about this topic. See: http://www.publications.parliament.uk/pa/ld200506/ldselect/ldeconaf/12/12we21.htm, accessed March 3, 2012.

IPCC writing teams and review editors (one of which was Tony McMichael) can influence the determination of which comments (and snippets of text) are *relevant* and which are not.

v) Issue fatigue and message dilution

It is an ironic but not unnoticed phenomenon that additional research may lead to greater rather than reduced uncertainty. Although there has been a marked growth in the CCH literature from SAR to TAR to AR4 (recall also Table 1), this has been accompanied, for the most part, by a weakening of the claims made by these assessments.

There is a further irony of the IPCC peer review process as currently conceived. It is true that this process is incredibly extensive and that the final outcome can rightly be said to have undergone a rigorous peer review. As such, any concluding statements can easily be viewed as "facts" – how could they not be if they have withstood so many opportunities for rebuttal? Yet at the same time, because writing teams have addressed each and every review comment, the strength of claims that can be attributed to these "facts" is often diluted. For this reason, others have argued that the messages from such assessments tend towards the banal:

> **R19:** I think there was probably a sense that IPCC is pretty cautious and conservative…and that it tends to be a lowest common denominator approach…they only publish things that practically everybody will have agreed to.

This might be particularly the case for claims made with "very high confidence": stating that malaria will both expand and contract (albeit to somewhat different extents and depending on location) is but one example. For future assessments, given the intense scrutiny that IPCC assessments face (e.g. Climategate, Himalayagate), could it be that writing teams will increasingly choose to err on the side of caution? As one interviewee stated when reflecting upon the increased attention that subsequent IPCC Assessments have received, "…in subtle ways, and not perhaps self-aware, the authors began to write in a defensive mode (**R22**)".

One additional issue related to the above is that parties with strong interests might put more energy into the IPCC review process than parties with less at stake. One Lead Author from AR4 certainly felt that the peer review process, and thus the final conclusions, was sub-optimal:

> **R8**: And I think the problem with the climate change chapters is that it did fail at the peer review - and don't attribute this to me, okay, but despite our best efforts, the health scientists wouldn't take the time to review it. We sent it to - and this was crucial - we sent it to all the malaria people, the water people, but they didn't review it, and if they didn't review it, what could we do?

That this occurred may partially represent the longstanding challenge that the CCH community has faced (3.2 – 3.4), namely their tenuous position within the broader public health landscape. Meanwhile, over 1,000 comments were received from other sources, and active opponents like Paul Reiter have taken the time to submit numerous comments.

Just as Lead Authors may have started to become disillusioned with the review process, so too have several of the more credible opponents (as opposed to the more blatantly obvious special interest or lobby groups):

> **R17:** But I didn't think that the IPCC was necessarily the best way to get my views across. I mean that genuinely. I know the people who are writing the chapters … and they'd interviewed me on certain chapters and... not really had any impact. People are quite determined to carry on with what they feel comfortable with.
>
> **R4:** And I saw the first draft of the IPCC health report … I was sent it to review by IPCC4. And I read it through quickly and quite frankly it was so badly written. It's difficult for somebody whose natural language isn't English to write but it was so badly written.

> I said, "Look, it would take me forever to edit this damn thing let alone to look at the science. I just can't. I don't have the time". And I turned it down.

A legitimate question for AR5 and beyond thus relates to the problem of issue fatigue among leading scientists. Weighing the relative merits of comments, always open to interpretative flexibility from the writing teams, could become much more problematic should both writing teams and peer reviewers become increasingly devoid of the most prominent scientists in the field and/or increasingly populated by special interests groups.[46]

5.3.5 Summary: CCH at the IPCC

Throughout 5.3, the extent to which IPCC assessments of the links between climate change and vector-borne diseases have mirrored, or masked, the wider controversies in the field has been examined. There are a few particularly important findings. The first is that it is clear that the personality and composition of the IPCC writing teams, which for health have had prominent participation by members of the CCH community (Chapter 3), have heavily influenced the final statements appearing in the SPMs as well as in the full technical chapters. Final IPCC statements on the links between climate change and vector-borne diseases have been, by and large, filtered through their perspective.

This is not to say that they have had a monopoly on the production of the texts – CCH opponents have been moderately successful in engaging IPCC processes to bring the climate-VBD debate into the assessments – but it is to say that the assessments would likely be very different in tone if it were the CCH opponents leading the work. The representation of the very complex climate-VBD controversies described in Chapter 4 favours the CCH proponents. As argued in

[46] This is not to denounce an idea dominant through much of this thesis, which is that CCH proponents and opponents themselves are driven, at least partially, by interests. However, when it comes to the broader spectrum of interests at play in climate change assessments, including oil industry lobbyists, who may act with levels of integrity orders of magnitude lower than the scientists described in this thesis (e.g. Oreskes & Conway, 2010).

5.3.4 concerning highland malaria, given that the venue is the IPCC, the terms of the controversy are also somewhat narrowed: a lengthy debate about the relative merits or use of certain climatic datasets for disease modelling is generally absent. In other words, the foundation upon which this research is based is much more solid in the safe confines of the IPCC than out in the peer-reviewed literature. The CCH community, it might be said, has benefitted from home-field advantage.

Despite this, as discussed in 5.3.4, there has been a clear dilution of the message coming out of the successive health chapters, and this could be for a variety of reasons. Champions of the IPCC might suggest that this is evidence of an ever-improving review process; cynics might suggest that it is a consequence of prior controversies – IPCC has become more cautious and conservative. Whatever the reason, and despite the trend towards more nuance in discussions about climate-VBD links, it must be pointed out that there are inconsistencies throughout the assessments. In TAR, dengue is not nearly as strongly linked to climate as is malaria; in AR4 it is the opposite. One of the reasons for this, discussed at length in 4.3.3 and 4.7.2, relates to varying visions of the future. IPCC would do well to make this more explicit in future assessments, given the significance that such visions have on how climate-VBD models are ultimately perceived. As one IPCC contributor noted:

> **R1:** The IPCC takes a far more cautious approach [than CCH opponents]. And it says if current social conditions are sustained, then it's most unlikely that malaria will return to Europe, even if the climate has changed. But it is not taking that for granted. And of course the answer is the different scenarios that we used as part of the modelling for the third and fourth assessment reports included a sort of variety of futures, which didn't necessarily assume that the advances that have held in the past, that have applied in the past, will have hold in the future.

This point cannot be emphasised enough. Aside from all of the other choices that modellers might make that can influence model outcomes – many involving judgments not particularly "scientific" – the choice of socioeconomic model and future assumptions are highly important. Moreover, as is generally well-known, the IPCC SRES (Special Report on Emissions Scenarios) underpinned climate change projections for TAR and AR4.[47] It is here important to reiterate that this, like many other aspects of IPCC work, is the outcome of a hybrid scientific and political process:

> **R6:** The SRES was a process led by the IPCC, so the governments did need to approve the SPM and then accept the underlying chapters. The governments did not directly approve the economic forecasts, but they did approve the key messages that came from those forecasts. Which could have had an indirect affect on the scenarios.

Particular socioeconomic visions of the future, vetted by governments, are used to underpin climate data models. When used in impacts research, such as for modelling climate-VBD futures, these visions become further embedded, meaning that some visions of the future are privileged at the expense of alternative visions.[48] [49]

This is but one example of the manner in which IPCC is, as Miller (2004) has suggested, both a consequence and an agent of co-production. As argued earlier in this chapter, the author nomination processes and the peer review processes are designed to balance political and scientific credibility, but as shown by following CCH through the four IPCC assessments, these processes are also much more likely to reinforce the dominant narratives surrounding climate change: namely, in the case of health, that climate change will lead to health outcomes, most of them adverse. Yet both of these claims, as shown in Chapter

[47] See: http://www.ipcc.ch/pdf/special-reports/spm/sres-en.pdf, accessed March 5, 2012.
[48] It is important to ask whether IPCC scenarios in general tend towards the optimistic or the pessimistic. Most interviewees who commented on this suggested that IPCC is more cautious about the future, as the quote above indicates, which is probably true for impacts modeling, but there is at least some reason to doubt this as concerns overall climate change projections. As one interviewee noted: "no government would approve something that suggests that there wouldn't be dramatic economic growth in their country (**R19**)".
[49] Recall the discussion from 1.4 about the ways in which risk tolerance and subjective assumptions are embedded in GCMs. It should be noted that the use of both climate models and SRES scenarios have been noted to limit the full range of extremes that could be potentially considered (e.g. Oppenheimer et al, 2007; van der Sluijs et al, 1998).

4, were the subject of intense controversy. Accepting them, it was suggested, has as much to do with subscription to a particular worldview as it does with the validity of the science. Thus, IPCC assessment reports have enforced and been reinforced by particular worldviews.

One final point needs to be made about the trajectory of climate-VBD knowledge claims through the four IPCC assessments. Although they have gradually become more nuanced, it was also argued in Chapter 3 that the impact of IPCC assessments has been quite significant:

> **R18:** …it's a great document to cite, because it has a lot of weight…because it's international and balanced.

As IPCC has been cited in a wide range of governmental policy documents, the impetus for action addressing the possible health impacts of climate change has grown. For example, a World Health Assembly Resolution and a European Commission Communication have specifically cited AR4.[50] Such citations tend to be used to support the claim that public health action needs to be taken to address climate change. Meanwhile, the more complicated and nuanced discussion embedded in the IPCC full chapters tend to be glossed over.

There is a further and final means through which IPCC stabilises CCH research and vice-versa: simply by assessing it. The medium is at least part of the message. If IPCC has studied possible health impacts of climate change, then the topic *must* be important and something *must* be done by public health agencies. Conversely, if these agencies, like WHO, have started to work on climate change, then the health impacts of climate change *must* be serious, adding yet another reason to more aggressively curb carbon emissions.

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[50] WHA 61.19 (http://apps.who.int/gb/ebwha/pdf_files/A61/A61_R19-en.pdf, accessed August 22, 2012). The DG SANCO produced a Commission Staff Working Document accompanying a White Paper on adaptation to climate change. At: http://ec.europa.eu/health/archive/ph_threats/climate/docs/com_2009-147_en.pdf, accessed August 22, 2012.

Conclusions

In the twenty-five years since the establishment of the IPCC, although numerous high-profile expert assessments of climate science have taken place in both international and national arenas, there remains the potential for controversy every time a new assessment is released. The complexity of the science alone, excluding broader political considerations, would be enough to cause this. Assessing the veracity of models (as well as their input data) necessitates considering many and vast uncertainties. In particular, the challenge of attribution in the present-day is notoriously difficult: even if one were to accept that climate change is "real", how could one know when such changes will actually start to occur? Is an abnormally mild winter, hot summer or heavy rainfall evidence of climate change, or simply year-to-year variation of the weather?

Recently, climate scientists have argued that some of the extreme weather events of past years can in fact be attributed to climate change. The prominent NASA scientist James Hansen, for example, recently published a paper in *Proceedings of the National Academies of Science* in which he claims that extreme events, such as a heatwave in Russia in 2010 and intense droughts in Texas and Oklahoma in 2011, "would not have occurred in the absence of global warming (Hansen et al., 2012)". Similar claims have been published in *Nature* in relation to the risk of flooding and heavy precipitation events in the Northern Hemisphere (Pall et al., 2011, Min et al., 2011). Such research can be expected to be highlighted by the upcoming IPCC 5th Assessment Report, which the former UN head of climate negotiations, Yvo de Boer, has claimed will "scare the wits out of everyone (quoted in Hannam, 2012)". It can also be reasonably expected that such claims will, once again, be both highly visible and heavily challenged, leaving policy-makers to decide: can IPCC scientists, with clear vested interests in climate change science, be trusted? Should societies start to act more aggressively to curb GHG emissions and begin to implement (potentially very expensive) adaptations to existing infrastructures?

Co-production and epistemic communities

In this thesis, the central question is as relevant to atmospheric science as it is to public health: what can be said about the credibility of climate change science by assessing how it is *made* to be credible? Following Jasanoff (2004a,b,c), I have suggested that examining this question can be fruitfully explored through a theoretical perspective inspired by co-production (2.4), because examining how science and its "internal" controversies relate to broader political structures reveals substantial insights into the myriad and often hidden ways that science and politics interact. Recall, for example, the climate-VBD controversy as presented by Brisbois and Harris Ali (2010), in which they identified some of the key actors and points of contention of the climate-vector-borne disease controversy, but did not lead to any particularly profound insights (4.2). As I argued, they did not draw attention to the broader contexts in which CCH research is both conducted and understood, and they also neglected to observe the controversy in other domains – crucially, as it manifested at the IPCC – leaving them to make the unfortunate admission that "there is more to the controversy than what we have been able to explain (Brisbois & Ali, 2010: 10)". In contrast, meanwhile, the literature on the role of scientific knowledge in governance – notably epistemic communities – usefully draws attention to the prominent role of experts within governance systems but tends to view expert knowledge as "objective" rather than as an interesting site for analysis (2.2).

Consistent in both approaches is that they neglect to consider that socio-political contexts and science are mutually constitutive. Analytically, this is overcome by explicitly considering *co-production*, which enables such connections by identifying specific thematic areas of attention (specifically for this thesis *emergence and stabilization, controversy, and intelligibility and portability* (2.4.4)) and by posing questions that these literatures tend to ignore. Thus analytical attention was drawn to the climate change and health community in Chapter 3. By acknowledging the types of ambiguities lacking in "traditional" epistemic communities theory (2.2), the co-production of CCH science and politics was examined at many different levels. In no sense could it be argued that the growth in credibility of CCH as a field of interest happened solely because of the "science". The trajectory of this research is the consequence of a broad range of factors: the pre-existing dominant framings of "global" climate change (1.3) and the EID worldview (1.4); pre-existing paradigmatic tensions within the field of epidemiology (3.2.1); the political and philosophical commitments of early CCH researchers (3.2.2, 3.3.1); the boundary work and awareness raising efforts of the CCH community (3.3.2); fractures and dynamics of the global health landscape (2.3, 3.3.3); and policies and processes at both international and national levels (3.3.4). Meanwhile, as CCH emerged and became stabilised, it influenced the creation of new institutes, units, and collaborations; new funding streams, policy initiatives and priorities for public health (3.3.5); and new topics to be considered and negotiated by the climate change community (3.3.5, Chapter 5).

Despite this relatively smooth trajectory for CCH, its academic and policy contexts are highly competitive, and it would have been surprising indeed if this research niche would not have been contested. Thus the longstanding controversy between CCH proponents and opponents (Chapter 4), which serves to further demonstrate just how important the extra-curricular activities pursued by the CCH community were in ensuring their discipline's stabilisation: this science was (and is) so heavily contested that one would have to be rather credulous to think that the emergence and stabilisation of CCH was related to purely "scientific" factors alone.

That CCH knowledge could be so intensely contested throws into question not only whether or not "consensus" surrounding CCH could be said to exist (to be discussed below) but also whether or not an epistemic CCH community could be said to exist. The analysis from Chapters 3, 4 & 5 demonstrates that in a scientific controversy, driven by contested norms and worldviews, actors may seek to control the message and membership of a given community. It is thus instructive to problematise the notion of "community" and "shared values": who gets invited to meetings like the WHO's *Madrid Consultation*, and why? Although I have referred to a CCH community in this thesis, dominated to a fair degree by CCH "proponents", the fact that a group of CCH "opponents" mobilised to contest the mainstream CCH "gospel" suggests that the homogeneity implied by the notion "epistemic community" may be somewhat illusory. This is demonstrated by the boundary work that took place to demarcate CCH "insiders" from "outsiders".

Models and controversy

To further emphasise this point, and to further probe the myriad ways in which judgements and values are incorporated into the production of CCH research, Chapter 4 examined specific climate-disease controversies at length. Thus it was noted how a group of disease ecologists and infectious disease specialists clearly resented the encroachment of the CCH community into their areas of expertise, getting "increasingly irritated (4.3.1)" with the prominence of some of the early climate-malaria models and how they eventually embarked upon their own research seeking to debunk the work of the mainstream CCH proponents. In the intense and often hostile debates that ensued, there are numerous instances of "accounting for error" (2.6.4), with explanations ranging from the wrong choice of method (e.g. biological as opposed to statistical models or vice-versa) (4.3.2), lacking of the appropriate scientific expertise (e.g. an epidemiologist instead of a biologist or ecologist) (4.3.4), choosing to work with the "wrong" climate input data (4.4), or the "wrong" number of variables in a model (4.5).

In all facets of the controversy, there were even deeper factors at play: the single-most important determinant of "model fit" appears to be the preferences of the person who conducted the work. Factors persistently driving controversy in climate-VBD research include subscription to different disciplines, each with strong interests in protecting their "niches", but also with different standards of proof, methodological approaches, and norms of interpretation (e.g. 4.7.1). More profoundly, discipline also appears to have influenced the ways in which researchers have accepted (or not) the credibility of data inputs from GCMs (e.g. 4.4), as well as the ways in which researchers view the future. Assumptions about socioeconomic "progress" and future societal resilience have been implicitly incorporated into climate change-disease models, to varying effects. In the climate-malaria controversy, for example, CCH opponents have tended to assume that socioeconomic circumstances would develop so as to mitigate any potential increase in malaria transmission in the future due to climate change, whereas CCH proponents assumed the opposite (4.3.4). Such assumptions are rarely, if ever, explicitly mentioned in the scientific literature; instead, they are implicitly incorporated into the technical design of models, such as through the way in which "presence" or "absence" points are defined (4.3.4, 4.5).

This hidden component of the climate-VBD controversy is deeply significant. As I mentioned in Chapter 2, as a researcher with multiple identities, one objective I have had for this research is to employ STS to be able to understand a controversy that has baffled many in the field for many years. Identifying how hidden socioeconomic visions of the future can affect climate-disease model outputs is an important contribution of this research because it introduces the question: which future are we talking about? Are two competing models based on the same climate scenarios and the same assumptions about socioeconomic development? This, in turn, begins to open-up these future-orientated assumptions to further deliberation rather than allowing a highly technical debate about the possible impacts of climate change on disease spread to obfuscate the fact that such assumptions exist. In varying public health contexts (e.g. international, national, sub-national), public health practitioners may then also contemplate whether or not disease modellers are the best people to be making inferences about how the future could evolve within their jurisdictions, and whether or not they might want alternative forms of expertise to be included in model-making activities.

Intriguingly, the possibility exists that multiple and potentially non-complementary visions of the future could be incorporated into the same climate-disease model, because GCM outputs are predicated on very particular socioeconomic assumptions embedded in emissions scenarios (1.4), which may well be at odds with the sorts of assumptions that some CCH modellers make when they decide to emphasise or de-emphasise future adaptive capacities (4.7). For example, a climate-disease model might based upon a SRES scenario anticipating positive economic growth, even where other assumptions built into the model contradict what one would generally expect to coincide with positive economic growth (such as a diminished public health infrastructure). More recent climate-disease modelling projects tend to compare outcomes across different SRES scenarios (e.g. Fischer et al, 2011, van Lieshout et al, 2004) and some have argued about the need to better consider uncertainties and even a broader range of SRES storylines when conducting climate change impacts research (Arnell, 2004, van Lieshout et al, 2004), but they nonetheless do not tend to assess whether different types of assumptions incorporated into their models are contradictory. Examining this matter in greater detail was beyond the scope of this thesis but could prove to be a productive avenue for further research.

At a broader level, there would appear to be scope for much more STS research examining the ways in which socioeconomic visions of the future are incorporated in the production of climate change emissions scenarios. As noted in 1.3, the development of emissions scenarios has tended to be dominated by "natural" scientists. Where studies have focused upon the limitations inherent in climate change scenarios, even highlighting the need for considering a broader range of potential socio-economic futures, such arguments do not tend to be accompanied by calls to incorporate complementary expertise, such as demographers, social scientists, or economists, into the research (e.g. Arnell et al, 2004). This appears to remain the case for a new process aimed at developing "the next generation" of climate change scenarios (Moss et al., 2010), for which it is noteworthy that IPCC – perhaps to avoid controversy and thus any further loss of credibility – has decided to not formally lead the current ongoing process aimed at producing a next generation of emissions scenarios. This process is nonetheless being driven, once again, by natural scientists (e.g. **R6**). As the outputs from this process will inform a whole generation of climate change impact models as well as IPCC assessments, climate change scenarios can be expected to remain a key site of co-production in climate change science and politics. They therefore remain a particularly interesting area for STS intervention, with many pertinent questions to pursue: what is the relationship between the mainstream political order of climate change and the content of these scenarios? How are certain worldviews – held by key actors in the scenario development process – embedded in and further constituted by "downstream" climate change impacts research? Which visions of the future do these models include and exclude and with which consequences?

Controversial contexts

Climate change impacts models, as we have seen through the climate-VBD controversies depicted in Chapter 4, were not simply amenable to controversy because of personal animosities between groups of researchers from different disciplines. The contexts within which such models are produced and warranted have also fostered and even helped to sustain controversy (and they have clearly also played a role in constituting the interests of these competing groups). I am specifically referring to both the evidential and the political contexts within which CCH research takes place. The evidential context, as we have seen (4.7.2), relates to the fact that a future-orientated science using data outputs as inputs is quite far along on the "uncertainty cascade". Should parties disagree, there are many potential battlegrounds for a scientific controversy. Meanwhile, one of the reasons that such a problematic evidential context exists is because the "political" context of CCH research necessitates future-modelling in the first place (e.g. 1.4). The political context also serves to raise the stakes of the controversy, because controlling the "scientific" argument means having greater sway in the "political" decisions that are made and implemented, including the allocation of limited resources for infectious disease control

(4.7.1). For these reasons, I argue that the very existence of an organisation like IPCC increases the likelihood that a controversy will be amplified, for IPCC offers a government-vetted and high-profile arena for turning certain scientific claims into "truth" (4.7.5, 5.2). Actors in a controversy clearly understand the value in "owning" an IPCC assessment process, for working as a CLA or LA has an important impact upon how a scientific issue will be presented – often as "consensus" – by IPCC (5.2, 5.3). In turn, the reach of IPCC reports is perhaps increasingly broad, influencing scientific disciplines as well as policy contexts (Vasileiadou et al, 2011).

I will return to the role of consensus in IPCC assessments below, but before doing so it is important to note that the climate-VBD controversy described in this thesis has been largely played out by "reputable" scientists; this is not a controversy driven by full-time special interests groups. David Rogers and Sarah Randolph were both professors at Oxford (they have recently retired), and even if some CCH proponents have accused the most radical of the CCH opponents, Paul Reiter, of having received money from the oil lobby[1], he nonetheless has a PhD in entomology and has held posts at the US CDC and the Pasteur Institute in Paris. The larger point here is that the analysis of Chapters 4 & 5 offers an important counter-point to the "Merchants of Doubt" thesis, in which it has been convincingly demonstrated that a wide range of advocacy groups have systematically deployed strategies designed to delegitimise the science underpinning climate change, often with the aim of protecting their (economic) interests and disrupting policy processes that might have led to binding limits of greenhouse gas emissions (Oreskes and Conway, 2010). Jasanoff has recently argued that there is a "bandwagon effect" around this thesis in climate change and even social science communities (Jasanoff, 2012). If this is true, then the *Merchants of Doubt* thesis may have already become a convenient truth for supporters of climate change, for it enables them to assert that consensus about climate change would have existed were it not for the

[1] Refer to note 85 in section 4.2.

special interests groups. This could have the unfortunate consequence of preventing more critical (and self-critical) and nuanced analyses of why there is disagreement about climate change in the first place (e.g. Hulme, 2009).

I do not wish to challenge the central premise of *Merchants of Doubt*, but simply to stress that it offers only one of many sides of the story. The side of the story presented in this thesis demonstrates that "real" scientists might also disagree with each other, for very understandable reasons, not only about whether or not climate change exists but also – perhaps even more importantly given that *most* scientists do accept that climate change is "real" and occurring – about whether or not some of the potential impacts of climate change are worth bothering about. These sorts of disagreements offer a productive avenue for additional STS research, as does further theoretical and empirical exploration of the implications of how (and by whom) the reputability of various actors in a given climate change controversy is co-constructed. Differentiating "reputable" from "disreputable", and by extension "insider" or "outsider", will doubtless continue to be a key area of activity for those institutions, like IPCC, that are working to police the boundaries and contents of climate science.

Fixing and formalising the IPCC

As it patrols the immensely complex and rapidly evolving climate change world, IPCC needs to be careful that its work does not come to be seen as partial or inaccurate. One of the principal objectives for this thesis (2.5.3) was to use the insights from a study of the CCH community and the controversies that they were involved in to reflect upon what this means for the credibility of climate change science and politics more generally – a topic much in discussion in the wake of Climategate (5.1) (e.g. Prins et al., 2010, IAC, 2010).

Recall that IPCC, as it was noted earlier, is the outcome of a long history of climate change politics and diplomacy and its principles and assessment processes have been established to "straddle the demands for scientific credibility and international political legitimacy (Agrawala, 1999: 158)". The

IPCC "performs a mix of functions, part scientific assessment, part policy advice, and part diplomacy – that demand external, as well as internal, accountability (Jasanoff, 2010: 696)". In its own rhetoric, IPCC strives to be "policy-relevant and yet policy-neutral, never policy-prescriptive".[2]

As should be clear by now, it is no easy feat to maintain an untarnished reputation as an honest broker for a highly uncertain and politicised science, especially when so many prominent stakeholders are openly antagonistic towards it. Unfortunately for IPCC, its complicated history, organisation and processes (5.2) – and the complicated science that it is assessing (5.3) – create many possibilities for critique. Foremost among them has been the selection of writing teams and experts and the peer review process (IAC, 2010), all of which exert a strong influence on the production of the content of IPCC Assessments (5.2, 5.3). As John R. Christy, a former IPCC Lead Author for TAR, recently argued in an opinion piece in *Nature* entitled "IPCC: cherish it, tweak it or scrap it?":

> Selected lead authors have the last word in the review cycle and so control the message, often ignoring or marginalizing dissenting comments. 'Consensus' and manufactured-confidence ensued (Hulme et al., 2010: 732).

The manner in which climate-VBD controversies were represented by IPCC assessments, notably TAR and AR4, confirms that the composition of writing teams significantly impacts the tone and content of the final text (5.3.3, 5.3.4). This raises some issues that require further discussion. The first is that if its assessments were truly "scientific", as IPCC would like to claim (e.g. "IPCC is a scientific body"[3]), then the perspective of selected lead authors should hardly matter at all:

> The conviction that science speaks objectively and disinterestedly means that one need have no qualms about excluding other people from decision-making

[2] http://www.ipcc.ch/organization/organization.shtml, accessed March 5, 2012.
[3] http://www.ipcc.ch/organization/organization.shtml, accessed March 5, 2012.

> since they would, in any event, have arrived at the same conclusions as oneself (emphasis removed) (Yearley, 1996a: 118).

Clearly, however, climate science is neither objective nor disinterested, and neither are IPCC assessments. One of the frequently cited challenges surrounding IPCC assessments has been that it can be hard to meet the requirements for gender and nationality balance among writing teams and still maintain a high scientific standard. In recent discussions about fixing the IPCC, suggestions for overcoming this problem tend to include improving the transparency with which writing teams are selected (IAC, 2010). This is at least sensible from the perspective of IPCC, for it would help allay concerns that IPCC dream-teams are being concocted by the IPCC secretariat. Somewhat more radical are the periodic calls to abolish these criteria altogether in the name of *better* science. As Jeff Price, a lead author in TAR and AR4 and director of climate change adaptation at the World Wildlife Fund, wrote in the same aforementioned *Nature* opinion article:

> Given the importance placed on these assessments, the most senior positions should be filled by the nominees most expert in the field, regardless of balance (Hulme et al., 2010: 732).

This, I would argue, would do little to contribute to the "scientific" credibility of an IPCC assessment but much to damage its "political" legitimacy, for such a stance is ahistorical – it neglects the many previous times that "southern" countries have felt misrepresented by "northern" technocratic conceptualisations of climate change (e.g. Agrawal and Narain, 1991). Under-represented nations can be expected to be much less likely to buy-in to IPCC conclusions and, potentially, UN climate change processes more generally. Recall that one of the very reasons for establishing IPCC was to engage UN Member States in climate change decision-making (5.2). Should the IPCC wish to alter the way in which it selects authors, it might instead consider broadening selection criteria to ensure that different disciplines and perspectives are considered alongside nationality and gender. Such a solution would certainly not ease the assessment process, but it could help to make it somewhat more robust. Thus far, as we have seen (e.g. 5.3), the IPCC has tended to run with its stronger allies, which makes it more vulnerable to claims that is not impartial.

It should, however, be questioned whether implementing stricter authorship selection criteria or, more broadly, establishing stricter assessment processes, is a useful approach for shoring-up the credibility of IPCC. That the debate has been restricted to such matters seems to suggest a continued belief that if only the IPCC were to act (and be perceived) *more* scientifically, then its credibility issues will disappear. It is telling, for example, that the IAC Committee that reviewed IPCC consisted of 10 scientists and 2 economists (Appendix E, IAC, 2010) who were given the mandate of reviewing the "processes and procedures of IPCC". Thus IAC advice to IPCC consists of items like: appointing an Executive Committee and Executive Director; ensuring that Review Editors are more forceful during the review process (which itself should be tightened up); developing a comprehensive communications strategy that would emphasise transparency and provide guidelines for determining, among other things, who can speak on the behalf of IPCC (IAC, 2010).

It is not difficult to see that these are pragmatic suggestions, because in following them IPCC assessments would at least *appear* to be more thorough and formal. Yet one is left wondering why IAC appears to have not more deeply contemplated the much messier contexts within which IPCC assessments are conducted. Recall from 5.2 that IPCC has tweaked its procedures throughout its history but, despite this, subsequent IPCC assessments have, if anything, become more heavily contested. As demonstrated in 5.3, regardless of how assessment procedures are drawn up, there is no way in which they can eliminate the human component that is so central to assessment-in-the-making. Nobody can determine who the *best* expert in a field is without enrolling their own perspectives and interests; there will always be the possible criticism that the selection of a writing team has been *impartial*. How, for example, would one select *the* pre-eminent expert from a highly polarised field, such as climate-VBD research? Similarly, the way in which a Review Editor might determine what counts as "the most significant review issues (IAC, 2010: 19)" is also dependent upon contingent human judgements. As such, parallel to the perils that surround an over-reliance on the *Merchants of Doubt* thesis, undue attention to processes and procedures might have the unintended consequence of preventing the recognition that the credibility of climate change science is neither fully dependent upon, nor can be fully reduced to, the types of organisations and procedures that are designed to assess it.

Consensus and the "assessment paradox"

To elaborate upon why this is problematic, it is instructive to consider the way in which IPCC presents "its reports to the world as the 'consensus' view of the leading climate change experts of the world (Hulme, 2008: 9)". Although some have accused IPCC of delivering conservative versions of "consensus", it is important to remember that the IPCC itself does not necessitate this. Recall from 5.2 that the IPCC *Principles* state: "It is important that Reports describe different (possibly controversial) scientific, technical, and socio-economic views on a subject, particularly if they are relevant to the policy debate (IPCC, 1999)". This is not lost on IPCC authors. As one interviewee, a Coordinating Lead Author for the upcoming AR5, noted:

> **R1**: As I understand the IPCC, its purpose is to provide some assistance to policy makers by assessing the current state of the science. So if the current state of the science is polarised or if there's serious disagreement then I would have thought the IPCC would be doing its job to represent that.

It is thus somewhat strange that so much importance in climate change discourse continues to be attached to "consensus" (e.g. Oreskes, 2004). Certainly, of course, consensus is an eminently useful tool, perfectly amenable to the wide range of settings, variably "scientific" or "political", in which climate science and IPCC assessments are mobilised. Indeed, the "consensus" on climate change has often been praised for facilitating a global agreement, however weak, about stabilising greenhouse gas emissions. Nonetheless, the idea of consensus needs to be problematised. As Gilbert and Mulkay convincingly observed nearly thirty years ago:

> ...participants' consensus accounts are highly variable and ... their meaning is linked to the interpretative situation in which they occur... it appears that, for the purposes of sociological analysis, a given field at a particular point in time *cannot be said to exhibit a specifiable degree of consensus*. Rather, the field must be said to exhibit varying degrees of consensus, depending on the discourse of those involved (original emphasis) (Gilbert and Mulkay, 1984: 140)

This observation appears to be particularly well-suited for analyses of science-policy interactions in climate change arenas. In their study of the surprising stability throughout time of the consensus-estimate range for climate change (1.5 °C to 4.5 °C), van der Sluijs, van Eijndhoven, Shackley and Wynne noted of IPCC:

> It has become an elaborate international means for securing consensus in the case for climate policy, although the notion of 'consensus' commonly employed is not straightforward. For instance, precisely what 'knowledge' is the object of that widely proclaimed consensus is open to debate (van der Sluijs et al, 1998: 293).

For the purposes of this thesis, the most significant aspect of their analysis is that the consensus-estimate was the product of a tacit agreement among a wide constellation of scientists and policy-makers which enabled local interpretative flexibility but also constrained the possible discourse. This restraining function still appears to exist. For example, multiple interviewees reflected upon the problem that although many climate scientists now believe that the trajectory for climate change is well beyond the climate sensitivity range, this is not something that is acceptable for discussion in the negotiating realm, such as at the COP meetings (e.g. **R6, R22**).

If it is indeed the case that there is the (perhaps increasing) tendency to err on the side of caution and avoid "reaching conclusions which are at all controversial (Van der Sluijs et al., 1998: 314)" – the so-called "lowest common denominator" approach – then there is the real risk that IPCC assessments will lose their policy relevance. This might be referred to as the *assessment paradox*. Assessments are conducted to provide policy-makers with the state-of-the-art in a given discipline, and yet owing to the perceived demands of the policy world for certainty, they can easily tend towards banal, generally agreeable statements that can be made with high confidence. At the same time, the information most needed may well be the opposite: radical statements with lower degrees of confidence. Recent pieces in *Science* and *Climatic Change* have alluded to this (even if they have been written by scientists who firmly believe in the concept of "consensus"):

> With the general credibility of the science of climate change established, it is now equally important that policy-makers understand the more extreme possibilities that consensus may exclude or downplay...
>
> ... increased transparency, including a thorough narrative report on the range of views expressed by panel members, emphasizing areas of disagreement that arose during the assessment, would provide a more robust evaluation of risk (Oppenheimer et al., 2007: 1506).
>
> In the past two decades, the IPCC's emphasis on consensus was necessary, and has served to help shift public opinion. Going forward, governments now need careful assessments of the relative risks and impacts of costs. Treatment of uncertainty will become more important than consensus if the IPCC is to stay relevant to the decisions that face us (Webster, 2009).

The IAC (2010) review of IPCC specifically addressed the articulation of uncertainty in relation to its examination of the activities of WGII in AR4. It highlighted some instances in which statements were made with "high confidence" even where limited evidence appeared to be available. The recommendation was to improve transparency and make use of a "level-of-understanding" scale, which states the degree of agreement and the amount of evidence (e.g. high agreement and much evidence; low agreement and much evidence; high agreement and limited evidence; etc.). Leaving aside the obvious subjectivity related to the use of such a scale, it can be argued that full transparency and the full representation of a wider range of uncertainties and viewpoints is unlikely to occur unless IPCC becomes more open about its intrinsically "hybrid" nature. I do not simply mean that IPCC would more readily admit that its activities are both scientific and political, but rather that it would take seriously the idea that the research it is assessing is also inherently scientific and political. Here the discussion about the incorporation of alternative socio-economic visions into climate-VBD models is once again particularly instructive (4.7, 5.3.3, 5.3.4). We have seen that the controversy over the possible impacts of climate change on dengue or malaria transmission is particularly intractable, but also that one important aspect of the controversy is that competing models can have very different assumptions about future socioeconomic circumstances and thus, by association, disease control competencies. Rather than present somewhat muted and distorted versions of the controversy in the health chapter, IPCC might be better off simply coming clean on *why* the different models differ. Ironically, this might have made the entire content more palatable to CCH proponents and opponents alike, since it could create space for both viewpoints. Presenting "consensus", meanwhile, particularly where the underlying science is heavily contested, has the potential to triply undermine the IPCC. First, because consensus often does not exist. Second, because it is debatable that consensus statements are of much use to policy-makers: a statement of consensus conceals differences of opinion and provides no information about the extent to which minority opinions have been considered. As Beatty & Moore (2010: 211) have commented:

> The very transparency that has been seen as a threat to scientific authority – by revealing a lack of unanimity – is actually a requirement for authority. It is the unanimity requirement that is the problem. What we want to know is that there was indeed disagreement, even to the end, but that by its own account the minority was heard.

It is perfectly reasonable for "experts" to review the same body of knowledge and, in spite of the science, come to different conclusions. Presenting a greater range of scientific findings (as well as the assumptions upon which they are based) might force organisations like IPCC to uncomfortably embrace their "hybridity", but this would also enable them to achieve a degree of "purification" (Latour, 1993) – and thus credibility – otherwise not possible. It would also leave more choice for a would-be policy-maker, thereby making assessments somewhat more policy-relevant and somewhat more policy-neutral. It is the opposite tactic – the "boundary protection" approach – which is more problematic. David Guston (2000) famously coined the term "boundary organization", but it has been quite rightly pointed out that this conceptualisation tends to reinforce rather than disintegrate the idea that the worlds of science and politics are separate (Miller, 2004). Nonetheless, the concept is influential and indeed it is not hard to imagine that IPCC has embraced it, perceiving as one of its key roles a certain policing of the boundaries between science and the political contexts that influence how and in which ways science is understood. Indefinitely maintaining such boundaries is however a task bound for failure: for a topic as influential, uncertain, and high-profile as climate change, there are simply too many opportunities for "impurities" to surface.

It is, of course, somewhat simpler to continue to assert that science and politics are separate, and to blame their intermingling for any controversies (or absences of unanimity) that arise: "When politics gets into science the truth tends to go out the window (**R4**)". In an *Ecologist* article entitled "Beyond Climategate: can we keep the politics and science of climate forecasting separate?", Vicky Pope, head of climate science advice at the UK Met Office, asserts the role for climate science:

> Our role is simply to supply objective evidence and to represent the uncertainty inherent in the scientific process. It isn't a question of right and wrong, but of trying to give a balanced assessment of what is certain and uncertain (quoted in Rees, 2011).

Should this type of idealistic thinking continue to be prevalent in climate change and climate change impacts communities, then there is good reason to suspect that climate change will continue to face a crisis of credibility. The deeply political, controversial and uncertain nature of climate change research has meant that time and time again, the rhetorically neat and tidy distinction between climate change science and politics has been exposed for the falsehood that it is. The IPCC and the current paradigm of "warranting" climate change science is an important part of this dynamic, for its high visibility and complicated procedures act as a magnifying glass through which any interested party can identify the contradictions between an official statement and the manner in which it was produced. As one interviewee reflected, the IPCC was established "to take the science out of the negotiations, so that you can craft some agreements that conform to the science (**R22**)". Instead, inevitably, IPCC assessments have exposed to the world the insight that scientific agreements conform to politics just as political agreements conform to science. Pretending otherwise is more likely to be a causal than a mitigating factor of the next climate science crisis.

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Appendix 1. Unpublished letter submitted to *Science* in 2000 in response to "The Global Spread of Malaria in a Future, Warmer World" (Martens et al, 2000).

In their report "The Global Spread of Malaria in a Future, Warmer World" (1), Rogers and Randolph (2000) favour a statistical modelling approach to estimate future changes in malaria risk above a biological approach. Their statistical-empirical approach, as opposed to the biological or process-based models, estimated changes not in the potential risk of transmission, but based on a climate-malaria equation derived from the actual contemporary geographic distribution of malaria. However, comparing biological and statistical is comparing apples with oranges. The biological and statistical models are thus estimating different parameters, and their results cannot be directly compared. We also question their analytical approach.

On a global scale, all current biologically based models show net increases in the transmission zone of malaria and changes in seasonal transmission under various climate change scenarios (2). Some local decreases in malaria transmission are also estimated to occur where declines in rainfall would limit mosquito survival and reproduction. This modelling of potential malaria transmission is a legitimate scientific exercise, seeking, in the first instance, information about how the intrinsic transmission-supporting properties of the world would vary under climate change.

The statistical modelling of actual transmission by Rogers and Randolph focused on the present day distribution of malaria and does not consider its historical distribution at extreme latitudes, for example in Europe or North America. Thus we wonder whether the model has satisfactorily captured the climate space needed to support malaria? Secondly, the climate in areas without malaria was described in a 10o band surrounding the malaria presence zones. These negative areas include major geographical barriers with extreme climates, such as the Sahara, Kalahari and Australian deserts, as well as the Andes and Himalayan mountains, all areas that are not representative of where most people live. Thus the climate model is probably defining those warm and wet conditions found in the tropics, the region where climate change will have the least impact, rather than providing a description of malaria potential.

Statistical modelling offers a different approach because it incorporates information about the current social, economic, technological modulation of malaria transmission. It assumes that those contextual factors will apply in future in unchanged fashion. This adds an important, though speculative, element of multivariate realism to the modelling - but the model thereby addresses a qualitatively different question from the biological model.

Both types of models have limitations and defects. The biological model assumes that there are known and generalisable biologically-mediated relationships. Also, in its current developmental state, such modelling has not yet attempted the horizontal integration of social, economic and technical change. The statistical model is based on socioeconomically censored data. It derives its basic equation from the existing (constrained) distribution of malaria in today's world and climatic conditions, and foregoes much information on the malaria/climate relationship within the temperate-zone climatic range. Yet this range is likely to be considerably important in relation to the marginal spread of malaria under future climate change.

The important task is to compare, and understand, the differing results in these two types of models. In this way the concepts and methods of estimating future climate impacts will evolve.